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EMPIRICAL TESTS OF THE POLLUTION HAVEN HYPOTHESIS WHEN ENVIRONMENTAL REGULATION IS ENDOGENOUS

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SUMMARY

The pollution haven hypothesis (PHH) posits that production within polluting industries will shift to locations with lax environmental regulation. While straightforward, the existing empirical literature is inconclusive owing to two shortcomings. First, unobserved heterogeneity and measurement error are typically ignored due to the lack of a credible, traditional instrumental variable for regulation. Second, geographic spillovers have not been adequately incorporated into tests of the PHH. We overcome these issues utilizing two novel identification strategies within a model incorporating spillovers. Using US state-level data, own environmental regulation negatively impacts inbound foreign direct investment. Moreover, endogeneity is both statistically and economically relevant.

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Supporting information may be found in the online version of this article.

1. INTRODUCTION

The precise relationship between environmental policy, the location of production, and subsequent trade flows remains an open and hot-button issue. Of particular concern is the so-called pollution haven hypothesis (PHH), whereby a reduction in trade barriers enables polluting multinational enterprises (MNEs) to relocate (at least some) production activities to areas with less stringent environmental regulation, thus altering both the spatial distribution of economic activity and subsequent trade patterns through the creation of havens for polluting firms. Kellenberg (2009, p. 242) states that 'the empirical validity of pollution haven effects continues to be one of the most contentious issues in the debate regarding international trade, foreign investment, and the environment'. Brunnermeier and Levinson (2004, p. 6) characterize the debate as 'particularly heated'.

Proper examination of this relationship is crucial for several reasons. First, the determinants of trade patterns and the spatial distribution of MNE activity are salient given the dramatic rise in foreign direct investment (FDI) relative to trade volumes over the past two decades. For example, global FDI inflows rose from less than \$600 billion in 2003 to roughly \$2.1 trillion in 2007 in nominal terms (UNCTAD, 2010). Due to the Great Recession, global FDI flows fell to \$1.1 trillion in nominal terms in 2009, but has since rebounded to \$1.7 trillion in 2011 (OECD, 2013). Aggregate inbound FDI stocks rose from \$2.1 trillion in 1990 to nearly \$18 trillion in 2009 and almost \$21 trillion in 2011 in nominal terms (UNCTAD, 2010; OECD, 2013). Moreover, the USA—the focus of this analysis—is the largest recipient of global FDI flows, receiving \$310 billion in FDI inflows in 2008, roughly \$100 billion more than the next largest host, Belgium (OECD, 2013). Even with the overall decline in FDI

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during the Great Recession, the USA remains the largest recipient of global FDI flows, receiving $234 billion in 2011. China was the second largest host in 2011, receiving $229 billion (OECD, 2013).

Second, if countries are able to attract (or deter) FDI by manipulating environmental regulations, then international coordination may be necessary to avoid Pareto-inefficient levels of regulation due to transboundary pollution or other spillovers (e.g. Levinson, 1997, 2003). Copeland (2008, p. 60) states that if the PHH is true then the 'exodus' of pollution-intensive firms to countries with lax regulation 'could create a political backlash' in stringent countries due to 'concerns about losses of jobs and investment'. In fact, this may even initiate a 'race to the bottom' in environmental standards. Moreover, as further discussed in Copeland (2008, p. 60), the PHH may also affect the stock of natural capital and 'exacerbate the effects of pollution on health and mortality' due to the lower income of countries with lax regulation. Within an individual country, the ability of environmental policy to influence capital flows across sub-jursidictions has implications for the debate over the appropriate level of governmental authority to establish environmental standards (e.g. Millimet, 2013).

Third, if countries are able to influence the location of MNE activity and ultimately trade patterns through environmental regulation, then bringing environmental policies under the purview of trade agreements may be necessary to realize the intended effects of such agreements (Ederington and Minier, 2003; Baghdadi *et al.*, 2013). Fourth, and related to this prior point, existing institutional structures such as the World Trade Organization (WTO) may be used to impede countries from choosing their desired environmental policies if such policies can be shown to impact trade flows between members (e.g. Eckersley, 2004). Finally, a detailed analysis of the PHH has broader implications for the general study of capital competition (e.g. Wilson, 1999).

Despite the high stakes, the existing literature has been unable to convincingly assess the empirical validity of the PHH for three reasons. First, environmental regulation is complex and multidimensional, making any empirical measure fraught with measurement error. Shadbegian and Wolverton (2010, p. 13) state: 'Measuring the level of environmental stringency in any meaningful way is quite difficult, whether at the national, state, or local level.' The difficulty arises from the fact that different regulations typically cover different pollutants, regulations may exist at multiple levels (e.g. federal and local), and monitoring and enforcement are imperfect. Along these lines, Levinson (2008, p. 1) states: 'The problem is not merely one of collecting the appropriate data; merely conceiving of data that would represent [environmental stringency] is difficult.' Xing and Kolstad (2002, p. 3) refer to the measurement of environmental regulation as 'no easy task' due to its 'complexity'. Moreover, depending on the empirical measure employed, the measurement error need not be classical and any bias may be accentuated by the reliance on fixed effects methods in the recent literature.

Second, even if an accurate measure of environmental regulation is available, it may be endogenous for other reasons (e.g. Levinson, 2008; Levinson and Taylor, 2008). For example, it may be correlated with unobserved determinants of location choice such as tax breaks or other firm-specific treatments, the provision of other public goods in addition to environmental quality (e.g. infrastructure), agglomeration, the stringency of other regulations such as occupational safety standards, corruption, local political activism, political institutions, etc. (see Arauzo-Carod *et al.*, 2010, for a review). In addition, reverse causation may be an issue. For instance, anticipation of low FDI inflows may drive reductions in environmental stringency; or an increase in FDI may increase the efficacy of industrial lobby groups (e.g. Cole *et al.*, 2006; Cole and Fredriksson, 2009). Conversely, as Keller and Levinson (2002, p. 695) state: 'Those states that do not attract a lot of polluting manufacturing probably do not enact stringent regulations—there simply is less need to worry about industrial pollution in states with less industrial activity, and those states that do attract polluting manufacturing may respond by enacting more stringent regulation.' Levinson (2010, p. 63) summarizes these arguments succinctly: 'International trade has environmental consequences, and environmental policy can have international trade consequences.'

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Third, existing studies of the PHH inadequately incorporate geographic spillovers. Recent theoretical models emphasize that the scale of MNE activity in one location depends not just on attributes of that location, but also on the attributes of other potential hosts. Moreover, the predicted direction of the cross-effects is not always in the opposite direction of the own-effects—a restriction that is implicit in discrete-choice models (e.g. Yeaple, 2003; Grossman *et al.*, 2006; Ekholm *et al.*, 2007; Baltagi *et al.*, 2007, 2008; Blonigen *et al.*, 2007, 2008; Arauzo-Carod *et al.*, 2010). Failure to account for geographic spillovers in empirical analyses of the PHH may lead to biased inference. This may be particularly problematic in the context of empirical analyses of inbound US FDI, since state-level environmental regulations have been shown to be strongly related to the regulatory stringency of neighboring states (Fredriksson and Millimet, 2002).

While these shortcomings, particularly the first and second, are well known, convincing solutions have proven elusive since standard fixed-effects models will not overcome these identification problems and valid exclusion restrictions have proved elusive. In this paper, we simultaneously address these three shortcomings while examining the spatial distribution of inbound US manufacturing FDI across the 48 contiguous states over the period 1977–1994. Geographic spillovers are incorporated in an unrestricted manner by including a spatially lagged counterpart for each state-level attribute. Measurement error, unobserved heterogeneity, and reverse causation concerns are then addressed utilizing two novel identification strategies designed to circumvent the need to identify valid exclusion restrictions in the usual sense.

The two approaches are similar in that each is based on an identification strategy utilizing higher moments of the data. The Klein and Vella (2009, 2010) and Lewbel (2012) approaches exploit conditional second moments to circumvent the need for traditional instruments. In the Lewbel (2012) approach, identification is achieved through the presence of covariates related to the conditional variance of the first-stage errors, but not the conditional covariance between first- and second-stage errors. Identification is achieved in the Klein and Vella (2009, 2010) approach by assuming that, while the errors are heteroskedastic, the conditional correlation between the errors is constant.

The results are striking. We consistently find (i) evidence of environmental regulation being endogenous when examining the behavior of pollution-intensive industries, (ii) a *negative* impact of own environmental stringency on inbound FDI in pollution-intensive sectors, particularly when measured by employment, and (iii) significantly *larger* effects (in absolute value) of environmental regulation once endogeneity is addressed. Moreover, neighboring environmental regulation is not an important determinant of FDI (although the estimates are relatively imprecise). However, spillovers from other attributes are present (although not the focus of this study), indicating the importance of incorporating spatial effects more generally in models of FDI determination. Thus, while the impact is not homogeneous, environmental regulation is a significant determinant of location choice by some MNEs at least at the regional level.

The remainder of the paper is organized as follows. Section 2 presents a brief literature review, concentrating on prior studies attempting to address endogeneity concerns. Section 3 describes the empirical methods, Section 4 discusses the data, and Section 5 presents the results. Finally, Section 6 concludes.

2. LITERATURE REVIEW

The literature assessing the empirical validity of the PHH has yet to reach a consensus due to the numerous complexities confronted by researchers. [1] Levinson (2008) effectively separates the literature into first- and second-generation studies. The first generation encompasses cross-sectional studies

[1] See Jaffe *et al.* (1995), Copeland and Taylor (2004), and Brunnermeier and Levinson (2004) for reviews of the literature.

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treating environmental regulation as exogenous. These studies typically found no statistically meaningful evidence in support of the PHH (and sometimes found counter-intuitive effects). The second generation predominantly encompasses panel data studies designed to remove unobserved heterogeneity invariant along some dimension (most often time, but occasionally across sectors differentiated by pollution intensity). Panel approaches, however, require environmental regulation to be strictly exogenous conditional on the (typically time-invariant) unobserved heterogeneity (and other covariates). A few studies within this second generation have attempted to relax this assumption and utilize traditional instrumental variable (IV) approaches. These second-generation studies typically find economically and statistically significant evidence in support of the PHH.

As mentioned, it is unlikely that existing panel studies are sufficient to yield unbiased estimates of the impact of environmental regulation on the location of economic activity and/or subsequent trade patterns. The omission of third-country effects, the omission of relevant variables that vary over time or differentially affect pollution-intensive and non-pollution-intensive sectors such as tax breaks and agglomeration effects, measurement error in proxies for environmental regulation, and dependence between current environmental regulation and past (or current) shocks to economic activity point strongly to violations of strict exogeneity (e.g. Henderson, 1997; List *et al.*, 2003; Cole and Fredriksson, 2009).

Recognizing this, several studies test the PHH utilizing traditional exclusion restrictions. These studies are summarized in Table I. At the risk of oversimplifying the literature, the instruments used generally fall within three categories. The first set includes lagged environmental regulation or lags of other covariates (Cole and Elliott, 2005; Jug and Mirza, 2005; Ederington and Minier, 2003). For such variables to represent valid instruments, the error term should not be serially correlated, which may be particularly unrealistic if measurement error is serially correlated or agglomeration effects are not accurately modeled. Both are distinct possibilities. Serial correlation in measurement error is likely due to the use of the same imperfect proxy over time. Agglomeration effects are not likely to be modeled perfectly given their complex nature due to multiple origins (e.g. domestic versus foreign and within and across industries) and nonlinearities (Arauzo-Carod *et al.*, 2010).

The second set includes instruments based on the geographic dispersion of industries (Levinson and Taylor, 2008; Cole *et al.*, 2005; Ederington *et al.*, 2004; List *et al.*, 2003). Specifically, the level of pollution emitted by *other* industries in the locations where a given industry tends to locate is used to generate instruments. For such variables to be valid instruments, the geographic distribution of industries must be exogenous. However, as with the first set of instruments, these instruments are likely to be correlated with the error term if agglomeration effects are not accurately modeled. In fact, the instruments fail the Sargan over-identification test at the $p < 0.01$ confidence level in Levinson and Taylor (2008). Similar instruments do fare better in Cole *et al.* (2005).

The final set of instruments include a variety of contemporaneous, location-specific attributes that are hypothesized to impact environmental regulation but not directly impact firm location decisions or trade patterns. Examples range from economic variables such as attributes of the agricultural sector, per capita income, and public expenditures to demographic variables such as the human development index, urbanization, infant mortality, population density, and schooling to political economy variables such as corruption, and proxies for industry lobby bargaining power. Kellenberg (2009) also utilizes some spatially lagged covariates as exclusion restrictions. Needless to say, one can plausibly argue in each case that such variables may also directly impact firm location or trade patterns, or be correlated with the error term due to non-classical measurement error or omitted geographic spillovers, agglomeration effects, or other sources of heterogeneity. Brunnermeier and Levinson (2004, p. 37), reviewing the literature at the time, state that 'as is always true of instrumental variable analyses, the instruments are open to critique'. That said, Kellenberg (2009) is noteworthy as the instruments fare well in terms of the usual specification tests.

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Despite the suspect validity of the identification strategies employed in these prior studies, rigorous specification testing is noticeably absent in many. A few discuss the strength of the first-stage relationship and/or conduct Hausman-type tests for endogeneity, but most neglect to test or even discuss why the proposed instruments should be exogenous or excluded from the second-stage equation for location choice or trade patterns; Levinson and Taylor (2008) and Kellenberg (2009) are notable exceptions. Nonetheless, these studies nearly universally obtain a more detrimental effect of environmental regulation on the behavior of pollution-intensive sectors once endogeneity is (attempted to be) addressed. Given this background, we now turn to our analysis.

3. EMPIRICAL ANALYSIS

3.1. Structural Model

Following Bergstrand and Egger (2007), Kleinert and Toubal (2010), Schmeiser (2013), and others, we estimate a gravity model of FDI. Accordingly, expected aggregate FDI flows or stocks from the rest of the world is given by

$$\mathrm{E}[\mathrm{FDI}_{it} \mid \widetilde{\eta}_i, \widetilde{\lambda}_t, x_{1it}, \ldots, x_{Kit}] = \widetilde{\eta}_i \widetilde{\lambda}_t \prod_{k=1}^{2K} x_{kit}^{\beta_k} \tag{1}$$

where FDI is some measure of MNE activity in location i and time t, x_{kit}, $k = 1, \ldots, 2K$, is a $2K \times 1$ vector of time-varying observable attributes of location i, and $\widetilde{\eta}_i$ and $\widetilde{\lambda}_t$ are location and period fixed effects, respectively. A simple extension of the theoretical model in Blonigen *et al.* (2008) provides guidance on the attributes belonging in equation (1). Specifically, we include variables reflecting 'own' and 'neighboring' production costs, trade costs, and market demand.[2] Environmental regulation is assumed to enter the empirical model as one of the determinants of production costs.

Neighboring variables are defined as follows. If x_{kit} represents an own attribute of location i at time t (such as location i's environmental stringency at time t), then also included in the model is the corresponding neighboring attribute, say $x_{k'it}$, given by

$$x_{k'it} = \sum\nolimits_{j \in \Omega} \omega_{ijt} x_{kjt} \tag{2}$$

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[2] In the interest of brevity, we do not provide a complete model, as the extension of Blonigen *et al.* (2008) is straightforward. Nonetheless, the basic structure is as follows. A parent country (e.g. the rest of the world), indexed by subscript 3, contains a single, horizontal firm undertaking production in the parent country as well as two host states in the USA, indexed by 1 and 2. Inbound FDI is strictly positive for both hosts. q_i denotes sales by the firm in location i, $i = 1, 2, 3$; Q_{ij} denotes production by the firm in location i sold in location j. Potential trade flows from the parent country to each of the host states is allowed, but no exports from the host states to the parent or between host states. The profit function of the multinational enterprise (MNE) is given by

$$\Pi^{\mathrm{MNE}} = \sum\nolimits_i \left[P_i(q_i; \Gamma_{0i}) q_i - C_i(Q_i; \Gamma_{1i}) - \gamma_i - \sum\nolimits_j t_{ij} Q_{ij} \right] - \Gamma$$

where $P_i(\cdot)$ is the inverse demand function, Γ_{0i} is a vector of demand shifters in i such that $P_i^{\Gamma_{0i}}, P_i^{q_i \Gamma_{0i}} > 0$, where superscripts denote derivatives, $C_i(\cdot)$ is total variable production cost associated with production in i such that $C_i^{Q_i}, C_i^{Q_i Q_i} > 0$, Γ_{1i} is a vector of variable production cost shifters in i, $t_{ij} \geq 0$ is trade costs of exports from i to j (where $t_{ii} = 0$), γ_i is the fixed cost associated with production in i, and Γ is a fixed cost parameter for the MNE. The objective of the MNE is to maximize profits with respect to Q_{11}, Q_{22}, Q_{31}, Q_{32}, and Q_{33}.
Comparative statics yield three insights. First, inbound FDI to a given host state is increasing (decreasing) in a state's own (neighboring) trade costs. Second, inbound FDI to a given host state is increasing in positive own and neighboring demand shifts. Third, inbound FDI to a given host state is decreasing (increasing) in a state's own (neighboring) production costs. Full details are available upon request.

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where ω_{ijt} is the spatial weight given by location i to neighbor j in period t and Ω includes the set of neighbors of location i. Thus, of the $2K$ attributes included in equation (1), K represents own attributes and the remaining K attributes represent the neighboring counterparts of these own attributes.

Log-linearizing a multiplicative error form of the model in equation (1) yields a standard fixed-effects panel data model. For clarity, we write the estimating equation as

$$\ln(\mathrm{FDI}_{it}) = \eta_i + \lambda_t + \sum\nolimits_{k=1}^{K} \beta_k \ln(x_{ikt}) + \sum\nolimits_{k=1}^{K} \delta_k \ln\left(\sum\nolimits_{j\in\Omega} \omega_{ijt} x_{kjt}\right) + \varepsilon_{it} \tag{3}$$

where β_k captures the effect of own attribute k, δ_k captures the effect of neighboring attribute k, η_i and λ_t are equivalent to $\exp(\widetilde{\eta}_i)$ and $\exp(\widetilde{\lambda}_t)$, respectively, and ε_{it} is a mean zero error term.[3,4]

Even if all elements in the regressors in the augmented model are strictly exogenous, estimation of equation (3) is non-standard given the introduction of the weights, ω. To proceed, the weights must be chosen a priori and this choice is necessarily ad hoc.[5] Because the true weights are unknown, we utilize four straightforward weighting schemes. First, we assign a weight of zero to non-contiguous neighbors and equal weights to all contiguous neighbors. In other words, $\sum_j \omega_{ijt} x_{jkt}$ simplifies to the mean of x_{jkt} in contiguous neighbors. Second, following Fredriksson and Millimet (2002), we adopt two regional breakdowns for the 48 mainland US states (see Appendix A). The use of regional weights is also motivated by the evidence in Glick and Woodward (1987) that foreign-owned affiliates in manufacturing tend to serve regional markets. For each regional breakdown, $\sum_j \omega_{ijt} x_{jkt}$ simplifies to the mean of x_{jkt} computed over all neighbors within the same region (again, giving each regional neighbor equal weight). The two regional classifications come from the US Bureau of Economic Analysis (BEA) and Crone (1998/1999). The BEA regional classification system was introduced in the 1950s

[3] Busse and Hefeker (2007) also provide support for a double-log specification for FDI using a Box–Cox model.

[4] One might consider augmenting the model in equation (3) with spatially lagged FDI (i.e. a spatial lag model). We pursue the current specification for two reasons. First, as discussed in Blume *et al.* (2010), identification becomes extremely difficult in models with spatially lagged covariates and dependent variable. Since our interest is in the effects of own and neighboring environmental regulation, we omit the spatially lagged FDI, implying our model should be viewed as a reduced form in this sense. Second, the theoretical FDI literature discussed previously implies specifications of the form in equation (3). Similarly, one might consider augmenting equation (3) with (temporally) lagged own FDI as a regressor (i.e. a dynamic panel data model) to capture agglomeration effects. While this is worth exploring in future work, we quickly ran into identification problems in the current data (even ignoring issues with the unequal spacing of the data discussed in the next section; see, for example, McKenzie, 2001). Thus we interpret the model as having omitted (a perhaps inadequate proxy for) agglomeration, contributing to the potential endogeneity of own and neighboring regulation.

[5] To explore the consequences of using incorrect weights, consider a simplified, cross-sectional model with a single covariate, x. Assume the 'true' model is given by

$$y_i = \alpha + \beta x_i + \delta \sum\nolimits_{j\in\Omega} \omega^*_{ij} x_j + \varepsilon_i$$

where x is the covariate and ω^*_{ij} is the 'true' weight placed on state j by state i. If the weights are misspecified such that the assumed weight is

$$\omega_{ij} = \omega^*_{ij} + \psi_{ij}$$

then substitution yields

$$y_i = \alpha + \beta x_i + \delta \sum\nolimits_{j\in\Omega} \omega_{ij} x_j + \left[\varepsilon_i - \delta \sum\nolimits_{j\in\Omega} \psi_{ij} x_j\right].$$

If ψ is mean zero and independent of x, then this is analogous to a standard random coefficients model (Swamy and Tavlas, 2003). In this case, $\psi_{ij} \neq \mathbf{0}$ generates heteroskedasticity which is actually exploited for identification by the estimators used in this paper. If ψ and x are not independent, then OLS estimates of δ will be biased in a non-trivial way in addition to the problem of heteroskedasticity. However, as in the usual case of measurement error, consistent estimation may still be possible via IV or other methods such as those explored here.

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and has never been amended. While this classification system is widely used by economists in studying regional economic activity, Crone (1998/1999) devised an alternative regional breakdown for US states using cluster analysis to group states according to similarities in economic activity. We refer to these weighting schemes as BEA and Crone regional weights, respectively. Finally, we utilize a weighting scheme based on (inverse) distances between US states. In this case, $\sum_j \omega_{ijt} x_{jkt}$ reduces to a weighted average of x_{jkt} computed over all other states; the weight attached by location i to neighbor j is $(1/d_{ij})/\sum_{j \neq i}(1/d_{ij})$, where d_{ij} denotes distance between i and j.

Even with specification of the weights, estimation of equation (3) is complicated by the fact that own and neighboring environmental regulation are likely correlated with the error term, ε, due to measurement error, spatial error correlation, unobserved heterogeneity, and/or reverse causation. As such, traditional fixed-effects estimates are not likely to yield consistent estimates of β and δ. Before turning to our identification approaches, we rewrite equation (3) more compactly, as well as introducing the first-stage equations, in order to make explicit the system of equations we are estimating. The system of equations is given by

$$\ln(\mathrm{FDI}_{it}) = X_{it}\Pi + \beta \ln(R_{it}) + \delta \ln\left(\sum\nolimits_{j\in\Omega} \omega_{ijt} R_{jt}\right) + \varepsilon_{it} \tag{4}$$

$$\ln(R_{it}) = X_{it}\Pi_R + \zeta_{1it} \tag{5}$$

$$\ln\left(\sum\nolimits_{j\in\Omega} \omega_{ijt} R_{jt}\right) = X_{it}\Pi_{SR} + \zeta_{2it} \tag{6}$$

where R is the proxy for environmental regulation, X includes all the other regressors from x in equation (3) except R (i.e. including the spatial terms and the state and time fixed effects), and ζ_1 and ζ_2 are the error terms in the first-stage equations assumed to be correlated with ε. All errors are assumed to be mean zero. Note that the model is not identified in the traditional sense since there are no exclusion restrictions in equations (5) and (6).

3.2. Lewbel's (2012) Approach

Lewbel's (2012) approach exploits the conditional second moments of the endogenous variables to circumvent endogeneity. This approach complements earlier work by Vella and Verbeek (1997), Lewbel (1997), Rigobon (2003), and Ebbes *et al.* (2009) and generates instruments that are valid under certain assumptions. Specifically, Lewbel (2012) shows that if the first-stage errors, ζ_1 and ζ_2, are heteroskedastic *and* at least a subset of the elements of X are correlated with the variances of these errors but *not* with the covariances between these errors and the second-stage error, ε, then the model is identified. As discussed in Lewbel (2012), these assumptions are satisfied by (but not limited to) systems of equations where error correlations across equations arise due to an unobserved common factor. In our context, as discussed below, measurement error in environmental stringency or an omitted index of crucial unobservables such as agglomeration and/or local political activism are plausible examples of such a common factor.

Formally, the Lewbel (2012) approach entails choosing $z_r \subseteq X$ such that

$$\mathrm{E}\left[z_r' \zeta_r^2\right] \neq 0 \tag{7}$$

$$\mathrm{E}\left[z_r' \varepsilon \zeta_r\right] = 0 \tag{8}$$

for $r = 1, 2$. If these assumptions are satisfied, then $\tilde{z}_r \equiv (z_r - \overline{z})\zeta_r$, $r = 1, 2$, are valid instruments as they are uncorrelated with the second-stage error given equation (8). Moreover, the strength of

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the instruments (i.e. their partial correlation with the environmental stringency variables) is directly related to the extent of heteroskedasticity in the first-stage errors given in equation (7).

For instance, if the errors in equations (4)–(6) contain a common (homoskedastic) factor, along with heteroskedastic idiosyncratic components (where the heteroskedasticity of ζ_r depends on z_r), then these assumptions will be satisfied. In other words, if we can rewrite the errors in equation (4)–(6) as

$$\varepsilon_{it} \equiv \kappa_{it} + \widetilde{\varepsilon}_{it}$$
$$\zeta_{rit} \equiv \varpi_r \kappa_{it} + \widetilde{\zeta}_{rit},\ r = 1, 2,$$

where κ is homoskedastic, $\widetilde{\zeta}_r,\ r = 1, 2,$ is heteroskedastic (with variance depending on z_r), ϖ_r are factor loadings, and $\widetilde{\zeta}_r$, $r = 1, 2,$ and $\widetilde{\varepsilon}$ are independent of each other and κ, then equations (7) and (8) are satisfied. Note that $\widetilde{\varepsilon}$ may be either homoskedastic or heteroskedastic. This data-generating process (DGP) is plausible if κ represents homoskedastic measurement error in environmental stringency, or a composite index of unobserved variables impacting both environmental stringency and FDI (such as those discussed previously) that is drawn from an identical distribution across observations. However, the idiosyncratic shocks to environmental stringency may be drawn from different distributions.

Note that measurement error in the weights does not, in general, satisfy the assumptions in equations (7) and (8). In the simplified, cross-sectional model described in footnote 5, $\kappa_i = -\delta \sum_{j \in \Omega} \psi_{ij} x_j$ which is heteroskedastic with variance depending on x. Thus setting $z = x$ would not satisfy the restriction in equation (8). However, if we extend the model from footnote 5 to allow for two covariates, as in

$$y_i = \alpha + \sum_{k=1}^{2} \beta_k x_{ki} + \sum_{k=1}^{2} \delta_k \sum_{j \in \Omega} \omega_{ij}^* x_{kj} + \varepsilon_i$$

then substitution yields

$$y_i = \alpha + \sum_{k=1}^{2} \beta_k x_{ki} + \sum_{k=1}^{2} \delta_k \sum_{j \in \Omega} \omega_{ij} x_{kj} + \left[\varepsilon_i - \sum_{k=1}^{2} \delta_k \sum_{j \in \Omega} \psi_{ij} x_{kj} \right]$$

where $\kappa_i = -\sum_{k=1}^{2} \delta_k \sum_{j \in \Omega} \psi_{ij} x_j$. In this case, if $\delta_1 = 0$, then x_1 may serve the role of z in order to derive an instrument for $\sum_{j \in \Omega} \omega_{ij} x_{2j}$ if it is related to the variance of the idiosyncratic portion of the first-stage error and uncorrelated with the covariance between the first- and second-stage errors due to the term $\delta_2 \sum_{j \in \Omega} \psi_{ij} x_{2j}$.

Homoskedastic measurement error in the covariates themselves (as opposed to measurement error in the weights) would also satisfy equations (7) and (8) as long as the variances of the idiosyncratic errors depend on x. Suppose now that the 'true' model is given by

$$y_i = \alpha + \sum_{k=1}^{2} \beta_k x_{ki}^* + \sum_{k=1}^{2} \delta_k \sum_{j \in \Omega} \omega_{ij} x_{kj}^* + \varepsilon_i$$

where x^* denotes the true value of x. Assume x_1^* is observed, but x_2^* is not. Instead, $x_{2i} = x_{2i}^* + \psi_i$ is observed, where ψ_i is homoskedastic. Substitution yields

$$y_i = \alpha + \beta_1 x_{1i}^* + \beta_2 x_{2i} + \delta_1 \sum_{j \in \Omega} \omega_{ij} x_{1j}^* + \delta_2 \sum_{j \in \Omega} \omega_{ij} x_{2j} + \left[\varepsilon_i - \beta_2 \psi_i - \delta_2 \sum_{j \in \Omega} \omega_{ij} \psi_i \right]$$

where $\kappa_i = -\beta_2 \psi_i - \delta_2 \sum_{j \in \Omega} \omega_{ij} \psi_i$. In this case, x_1^* and $\sum_{j \in \Omega} \omega_{ij} x_{1j}^*$ may serve the role of z in order to derive instruments for x_2 and $\sum_{j \in \Omega} \omega_{ij} x_{2j}$ as long as x_1^* and $\sum_{j \in \Omega} \omega_{ij} x_{1j}^*$ are related to the variance of the idiosyncratic portion of the first-stage errors and uncorrelated with the covariance between the first- and second-stage errors due to the presence of ψ_i.

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In the analysis, we use the Koenker (1981) version of the Breusch–Pagan test for heteroskedasticity to identify variables significantly related to the first-stage error variances. We include a subset of x in z_1; the spatially lagged counterparts of these variables are included in z_2 (discussed below). The instruments, $\tilde{z}_r$, are then created by replacing ζ_r with its estimate obtained from (consistent) ordinary least squares (OLS) estimates of the first stage. As z_1 and z_2 are vectors in our implementation, the models are over-identified. Thus the usual battery of specification tests in models estimated via instrumental variables are available. Finally, note that after construction of the instruments estimation is carried out using generalized method of moments (GMM). See Appendix B for further estimation details.

3.3. Klein and Vella's (2009) Approach

The next identification strategy is based on a parametric implementation of the estimator proposed in Klein and Vella (2009, 2010) and expanded upon in Farré *et al.* (2013). To proceed, recall that we are still working with the same system of equations given in (4)–(6). However, rather than invoking the assumptions given in equations (7) and (8) concerning the errors, the following assumptions are made:

$$\varepsilon_{it} = S_\varepsilon(z_{it})\varepsilon^*_{it} \tag{9}$$

$$\zeta_{rit} = S_r(z_{it})\zeta^*_{rit}, \quad r = 1, 2 \tag{10}$$

$$S_\varepsilon(z_{it})/S_r(z_{it}), \quad r = 1, 2, \text{ varies across } i \tag{11}$$

$$\mathrm{E}\left[\varepsilon^*_{it}\zeta^*_{rit}\right] = \rho_r, \quad r = 1, 2 \tag{12}$$

where ε^*_{it} and ζ^*_{rit} are homoskedastic errors and $z \subseteq X$. Thus at least some of the errors are required to be heteroskedastic in such a way that the ratio $S_\varepsilon(z_{it})/S_r(z_{it})$, $r = 1, 2$, varies across observations.[6] However, the conditional correlation, ρ_r, $r = 1, 2$, between the underlying homoskedastic portion of the errors must be fixed. Note that, while the three heteroskedasticity terms—$S_\varepsilon(z_{it})$ and $S_r(z_{it})$, $r = 1, 2$—are written as a function of the same set of covariates, z, this need not be the case. There are no restrictions on which variables may enter each of these terms.

Klein and Vella (2010) give some examples of DGPs satisfying these assumptions. One such case arises if there exists a common factor, as in the Lewbel (2012) approach. However, here the common factor enters multiplicatively and may itself be heteroskedastic. Specifically, if we can write the errors as

$$\begin{aligned}\varepsilon_{it} &= S_\varepsilon(z_{it})\kappa_{it}\widetilde{\varepsilon}_{it}\\ \zeta_{rit} &= S_r(z_{it})\kappa_{it}\widetilde{\zeta}_{rit}, \ r = 1, 2\end{aligned}$$

where κ is the common factor and $\widetilde{\varepsilon}$ and $\widetilde{\zeta}_r$ are mean-zero, independent of X and κ, and have a constant correlation given by ρ_r, then equations (9)–(12) are satisfied.

Referring back to equation (12), it is worth considering what this identification condition implies. One possible interpretation includes viewing ε^*_{it} and ζ^*_{rit}, $r = 1, 2$, as correlated measures of agglomeration (see footnote 4). Agglomeration may affect environmental stringency due to the scale effect

[6] In the Lewbel (2012) approach, $S_\varepsilon(z_{it})/S_r(z_{it})$, $r = 1, 2$, varies across observations as well, but does so because $S_r(z_{it})$ varies. Here, the source of variation may be due to $S_\varepsilon(z_{it})$.

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of pollution-generating activity. However, the impact may depend on state-level attributes, z_{it}. For instance, states may differ, according to z, in their ability to limit the environmental damage of agglomeration. Improved public infrastructure, for example, may help ameliorate the environmental impacts of agglomeration. Smaller population size may encourage greater collective action by environmental groups through a reduction in free-riding (Olson, 1965). As a result, the policy response from a change in agglomeration may differ across locations according to such attributes. Similarly, own agglomeration may impact FDI through economies of scale, but the effect may again be mitigated or enhanced by state-level attributes. States with certain characteristics, such as market proximity or low wages, may be better positioned to realize positive agglomeration externalities. Finally, neighboring agglomeration may adversely impact FDI by improving the desirability of neighboring locations, with the magnitude of the effect depending on neighboring attributes that better position localities to take advantage of positive agglomeration externalities. However, once we *condition* on these state-level attributes, the returns to own and neighboring agglomeration, ρ_1 and ρ_2, respectively, are constant. Thus the returns to agglomeration are allowed to vary spatially and temporally, depending upon state characteristics. Assumption (12) simply states that returns are constant once these state attributes are accounted for. While not testable, this seems plausible. Moreover, as emphasized in Farré *et al.* (2013), Klein and Vella (2010) show that the assumption of a constant *conditional* correlation is consistent with many DGPs.

Continuing, we parametrize $S_\varepsilon(z_{it})$ and $S_r(z_{it})$ as

$$S_\varepsilon(z_{it}) = \exp\left(\frac{z_{\varepsilon it}\theta_\varepsilon}{2}\right) \tag{13}$$

$$S_r(z_{it}) = \exp\left(\frac{z_{rit}\theta_r}{2}\right),\ r = 1, 2 \tag{14}$$

where z_r includes additional covariates beyond those employed in the Lewbel (2012) approach.[7] Using the Koenker (1981) version of the Breusch–Pagan test, we identify an additional vector of covariates likely to be related to the structural error variance in the FDI equations, z_ε.

With this set-up, equation (4) may be rewritten as

$$\ln(\text{FDI}_{it}) = X_{it}\Pi + \beta\ln(R_{it}) + \delta\ln\left(\sum\nolimits_{j\in\Omega}\omega_{ijt}R_{jt}\right) + \rho_1\frac{S_\varepsilon(z_{it})}{S_1(z_{it})}\zeta_{1it} + \rho_2\frac{S_\varepsilon(z_{it})}{S_2(z_{it})}\zeta_{2it} + \widetilde{\widetilde{\varepsilon}}_{it} \tag{15}$$

where $\rho_1\frac{S_\varepsilon(z_{it})}{S_1(z_{it})}\zeta_{1it}$ and $\rho_2\frac{S_\varepsilon(z_{it})}{S_2(z_{it})}\zeta_{2it}$ are control functions and $\widetilde{\widetilde{\varepsilon}}_{it}$ is a well-behaved error term. Given the functional form assumptions in equations (13) and (14), equation (15) can be estimated by nonlinear least squares (NLS) in a number of ways. Standard errors are obtained via bootstrap. See Appendix B for further estimation details.

4. DATA

All of the data except interstate distance, d_{ij}, come directly from Keller and Levinson (2002); thus we provide only limited details.[8] Summary statistics are provided in the online Appendix C

[7] The Lewbel (2012) approach does not require one to identify *all* covariates satisfying equations (7) and (8). All we require is a sufficient number of (valid) instruments to identify the model. In fact, too many instruments may have undesirable effects particularly if some instruments are weak (Wooldridge, 2002). However, the Klein and Vella (2009) approach requires a consistent estimate of $S_r(z_{it})$, $r = 1, 2$.

[8] The data on interstate distances are from Wolf (2000) and have been used in Millimet and Osang (2007) and elsewhere.

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(supporting information).[9] The data cover the 48 contiguous US states from 1977 to 1994, omitting 1987 due to missing data on abatement costs. The measures of FDI include the value of gross property, plant, and equipment (PP&E) of foreign-owned affiliates for all manufacturers, as well as just for the chemical sector (1992–1994 omitted), and employment at foreign-owned affiliates for all manufacturers, as well as just for the chemical sector (1992–1994 omitted).[10,11] The chemical sector (SIC 28) is analyzed in isolation given that FDI in these industries is most likely to be responsive to spatial variation in environmental stringency given the pollution-intensive nature of production (Ederington *et al.*, 2005).

Consistent with figures reported elsewhere, inbound FDI stocks increased tremendously over the sample period. Aggregate manufacturing PP&E increased over tenfold from 1977 to 1994, from roughly \$20 million to nearly \$300 million (in 1982 US\$). A similar increase occurred in the chemical sector from 1977 to 1991, from roughly \$10 million to \$90 million. Employment grew at a slower, but still substantial rate, increasing from roughly 675,000 to almost 2.3 million in aggregate manufacturing and from 190,000 to 500,000 in the chemical sector.

In the theoretical model of inbound FDI discussed above (see footnote 2), determinants of FDI include trade costs, cost and demand shifters, and parent country attributes. Here, total road mileage and state effects capture time-varying and time-invariant (e.g. distance to ports) differences in trade costs across states. Population and market proximity (a distance-weighted average of all other states' gross state products) reflect market size and demand shocks. Relative abatement costs (RAC), unemployment rate, unionization rate, average production worker wages across the state, land prices, energy prices, and tax effort (actual tax revenues divided by those that would be collected by a model tax code, as calculated by the Advisory Commission on Intergovernmental Relations) capture variation in production costs and resource availability.[12,13] RAC is the proxy for environmental regulation. This measure is attributable to Levinson (2001) and represents the ratio of actual state-level abatement costs to predicted state-level abatement costs, where the predicted value is based on the industrial composition of the state. Consequently, higher values indicate relatively more stringent environmental protection. The index varies over time and across states. Finally, since FDI is aggregated across all countries outside the USA, time effects capture parent country attributes. All variables are expressed in logarithmic form with the exception of the unemployment and unionization rates. In addition, following equations (1)–(3), we form the spatially lagged variables first and then take logs, again with the exception of spatially lagged unemployment and unionization rates.

Prior to continuing, it is important to note that the Spearman rank correlation between RAC and total manufacturing FDI as measured by PP&E is positive ($\rho = 0.11$, $p = 0.003$); the correlation is even stronger when only considering the chemical sector ($\rho = 0.13$; $p = 0.001$). Neither correlation is statistically significant using employment to measure FDI. Moreover, as shown in Keller and Levinson (2002), total manufacturing FDI as measured by employment (and PP&E) increased by more over the sample period in the 20 states experiencing the largest increase in RAC than in the 20 states

[9] See http://faculty.smu.edu/millimet/pdf/mr_AppendixC.pdf.

[10] For each dependent variable, the sample represents an unbalanced panel where the number of observations for total manufacturing PP&E (employment) are 811 (814); for chemical sector PP&E (employment), the sample size is 563 (621).

[11] Following Keller and Levinson (2002), Cole and Elliott (2005), Kellenberg (2009), and others, we analyze FDI stocks. The inclusion of fixed effects in the model, however, implies we are utilizing the temporal variation in stocks to identify the parameters.

[12] Although ignored by much of the prior literature, one might be concerned about whether other covariates besides own and neighboring environmental regulation are not strictly exogenous. For example, Eskeland and Harrison (2003) treat some covariates as endogenous in a model of FDI shares by industry (but treat pollution abatement costs as strictly exogenous). Unfortunately, this is beyond the scope of the current study.

[13] Note that the unemployment and unionization rates enter equation (3) in level form.

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experiencing the largest decline in RAC. In addition, Table C1 in the online Appendix shows that mean total manufacturing FDI as measured by PP&E is higher when RAC exceeds one (indicating more stringent environmental regulation), as well as for the chemical and non-chemical sectors considered separately. However, mean total manufacturing employment, as well as in the chemical and non-chemical sectors, is lower in states with RAC greater than one. In any event, finding statistical evidence consistent with the PHH, particularly using data on PP&E, would appear to require the existence of significant selection (on either observed or unobserved variables) into more stringent RAC.

5. RESULTS

5.1. Lewbel's (2012) Approach

The baseline results are presented in Tables II and III. Table II contains the results for the chemical sector only; Table III assesses total manufacturing. Panel A in each table measures FDI using PP&E; panel B measures FDI using employment. Five specifications are estimated in each panel. Specification 1 omits all geographic spillovers. Specifications 2–5 include such spillovers, where specification 2 uses the contiguous weighting scheme, specifications 3 and 4 use the BEA and Crone regional weighting schemes, respectively, and specification 5 uses the distance-based weighting scheme. The estimates obtained using Lewbel's (2012) approach are given under the column labeled 'IV'. OLS estimates are presented for comparison, where the specification 1 results are identical to Keller and Levinson (2002). [14]

To generate the instruments, we include three variables in z_1 and z_2. Specifically, z_1 includes land prices, market proximity, and total road mileage; z_2 includes the spatial lags of these variables. [15] It is interesting to note, with further examination, that land prices and total road mileage are associated with a lower variance of ζ_1; neighboring land prices and total road mileage (market proximity) are associated with a lower (higher) variance of ζ_2. In Keller and Levinson (2002), land prices and total road mileage are negatively associated with FDI inflows, whereas market proximity is positively related. Thus the pattern of heteroskedasticity is consistent with the notion that states with less favorable attributes for attracting FDI minimize the volatility in another attribute—environmental stringency—that may adversely impact inbound FDI.

Turning to the results, we obtain five salient findings. First, the OLS estimates are negative and statistically significant in the vast majority of cases. The main exception is when examining FDI as measured by employment in total manufacturing (panel B, Table III). In addition, the OLS estimates are fairly stable across the five specifications; neighboring environmental regulation is statistically

[14] We only display the point estimates for own and neighboring environmental regulation to conserve space. Full estimation results are available upon request. However, Tables C2–C5 in the online Appendix contain the complete first-stage results, while Tables C6 and C7 report the full set of coefficient estimates on the covariates for specifications 1, 3, and 5 for the chemical sector. Heteroskedasticity-robust standard errors are used (Baum *et al.*, 2007). Note that these standard errors ignore the estimation error of the instruments. While Lewbel (2012) derives the appropriate asymptotic standard errors based on independent and identically distributed observations, we prefer heteroskedasticity-robust standard errors. In brief simulations (details available upon request), we find little difference in the empirical distributions of the estimator when the 'true' instruments are used rather than the estimated instruments.

[15] According to the Koenker (1981) version of the Breusch–Pagan test for heteroskedasticity of the first-stage error for own environmental regulation, land values, market promixity, and total road mileage have test statistics of 41.44, 42.69, and 11.92, respectively, when using PP&E for aggregate manufacturing. When using PP&E for the chemical sector alone, the test statistics are 7.43, 15.23, and 17.44. The test statistic is distributed χ^2_1 and we reject the null of homoskedasticity in each case at the $p < 0.01$ level. The tests of heteroskedasticity of the first-stage error for spatially lagged environmental regulation yield test statistics of 47.91, 46.10, and 10.70 for neighboring land values, neighboring market promixity, and neighboring total road mileage, respectively, when using distance-based weights and PP&E for aggregate manufacturing. When using PP&E for the chemical sector alone and distance-based weights, the test statistics are 7.85, 14.45, and 15.96. The test statistic is again distributed χ^2_1 and we reject the null of homoskedasticity in each case at the $p < 0.01$ level. See also Table IV. Additional results—using other weighting schemes or for other covariates—are available upon request.

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significant only in specifications 2 and 3 when assessing employment in the chemical sector (panel B, Table II). Inclusion of the spatial effects has little effect on the estimated marginal effect of own environmental regulation.

Second, the Lewbel (2012) identification strategy works well as determined by the usual IV specification tests when geographic spillovers are omitted (specification 1) as well as in the majority of cases when spatial effects are included. Specifically, we reject the null that the model is under-identified at the $p < 0.01$ confidence level in every case using the Kleibergen–Paap (2006) rk statistic, and the Kleibergen–Paap F-statistic is reasonably large with the possible exception of specification 4 when examining the chemical sector. In addition, we fail to reject the validity of the instruments using Hansen's J-test in all but two cases at the $p < 0.10$ confidence level for specifications 1, 3, 4, and 5. Thus the specification tests support the identifying assumptions invoked in Lewbel's (2012) approach. Third, when focusing on the cases that pass the specification tests, we reject exogeneity of own and neighboring environmental regulation in the majority of cases for the chemical sector. There is much less support for endogeneity when examining total manufacturing.

Fourth, turning to the point estimates in the cases that pass the specification tests for the chemical sector (Table II), the GMM estimates are statistically significant at least at the $p < 0.10$ confidence level using either the traditional approach or the Anderson and Rubin (1949) test robust to weak instruments in most cases—often statistically significant at the $p < 0.01$ confidence level, particularly when measuring FDI using employment (panel B). Moreover, the point estimates are larger in absolute value compared to OLS; however, the standard errors are also roughly two to three times larger. For instance, in specification 1 in panel B, we obtain an elasticity of employment in the chemical sector with respect to environmental stringency of about −0.40 (standard error = 0.07) when using OLS. Thus a 10% increase in environmental stringency for the mean state, which is about one-quarter of a standard deviation, leads to a 4% decline in employment in foreign-owned affiliates in the chemical sector, or about 300 lost jobs for the mean state. The GMM estimate roughly doubles to −0.84 (standard error = 0.16).

The fact that the IV estimates suggest a stronger adverse effect of environmental regulation is consistent with many of the papers listed in Table I, such as Xing and Kolstad (2002), Ederington and Minier (2003), Fredriksson *et al.* (2003), Levinson and Taylor (2008), and Cole and Fredriksson (2009). For example, Xing and Kolstad (2002) obtain a point estimate for FDI in the chemical sector that is more than three times larger once environmental regulation is treated as endogenous. Ederington and Minier (2003) obtain an elasticity estimate over 60 times greater once environmental regulation is treated as endogenous. Cole and Fredriksson (2009) obtain IV estimates opposite in sign from the OLS estimates and 10–75 times larger in absolute value. Furthermore, the magnitude of our estimates are on a par with those obtained in Kellenberg (2009) when examining the chemical sector in isolation. Finally, neighboring environmental regulation is statistically significant in specification 3, but not specifications 4 and 5.

To put the magnitude of the effects in further context, consider the results in panel B, specification 5. Ohio in 1991 had 17,600 workers in foreign-owned affiliates in the chemical sector. The value of its RAC index was 0.86, making it a fairly lax state according to the index. The *ceteris paribus* effect of Ohio increasing its RAC at the time to match California (1.00) is estimated to entail a decline in employment in foreign-owned affiliates in the chemical sector from 17,600 to roughly 15,600. In contrast, the OLS estimate implies a decline to only about 16,600.

In terms of the total manufacturing results (Table III), we often fail to reject exogeneity, as noted previously. Moreover, adding the spatial effects has little influence on the estimates from Keller and Levinson (2002); Kellenberg (2009) obtains a similar finding. One noteworthy finding, however, occurs in specification 2 when examining PP&E (panel A). Here, we do reject exogeneity and the IV point estimates for own and neighboring environmental regulation are statistically significant at the $p < 0.05$ level. Notwithstanding this case, we generally obtain much smaller and statistically

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insignificant estimates when examining manufacturing as a whole. This is consistent with prior evidence that the impact of environmental regulation (as well as the statistical properties of estimates) depends on the pollution intensity of the industry (e.g. Ederington *et al.*, 2005; Jug and Mirza, 2005; Henderson and Millimet, 2007; Mulatu *et al.*, 2010).

In sum, the Lewbel (2012) approach indicates an economically and statistically significant adverse impact of own environmental stringency on inbound FDI in the pollution-intensive chemical sector, particularly in terms of employment, once endogeneity is addressed. However, there is little evidence that neighboring environmental regulation matters, nor is there evidence of a deleterious effect of own or neighboring environmental regulation on inbound FDI for manufacturing as a whole. The downward (in absolute value) bias of OLS estimates for the chemical sector may be attributable in part to measurement error and in part to unobservables positively correlated with both environmental regulation and FDI inflows. For instance, Becker (2011) finds that there is significant variation in environmental compliance costs across counties within states; roughly one-third of counties differ significantly from their state average. Thus significant attenuation bias due to measurement error is clearly plausible. Similarly, a multitude of unobservables (such as investments in other public goods or agglomeration effects), as well as omitted within-state variation in the observables included in the analysis, can explain the bias in estimates obtained under the assumption of strict exogeneity. We next turn to the Klein and Vella (2009) approach for comparison.

5.2. Klein and Vella's (2009) Approach

The results from Klein and Vella's (2009) approach are also presented in Tables II and III under the column labeled 'CF' (for control function). As noted above, we include an expanded set of variables in z_1 and z_2 relative to Lewbel's (2012) approach. Specifically, we set $z_1 = z_2 = z$, where z includes land prices, total road mileage, market proximity, population, unemployment rate, unionization rate, and the spatial lags of these variables. Allowing for heteroskedasticity in the second-stage error, ε, we include average production worker wages, population, and market proximity in z_ε when examining FDI in the chemical sector; market proximity only is included when examining total manufacturing FDI.[16]

Before discussing the point estimates, it is important to note that our specification of $S_r(z_{it})$, $r = 1, 2$, and estimation procedure appears to work well. In particular, while we always reject the null of homoskedastic errors in both first-stage equations using the Koenker (1981) test at the $p < 0.01$ level, we predominantly fail to reject the null after transforming the data by $1/\sqrt{\widehat{S}_r(z_{it})}$. As reported in Table IV, we continue to reject the null of homoskedasticity (albeit at lower levels of confidence) in the model for neighboring environmental regulation in specifications 3 (BEA regional weights) and 5 (distance-based weights). We only reject the null of homoskedasticity in the model for own environmental regulation once (at the $p < 0.10$ level) when spillovers are included.[17]

Turning to the results for the chemical sector in Table II, the point estimates for own environmental regulation are fairly stable across the five specifications, particularly in panel A (PP&E).[18] Moreover, the estimates are never statistically significant at the $p < 0.10$ confidence level due to the relatively large standard errors except in specification 3 when examining employment (panel B). Neighboring environmental regulation is also rarely statistically significant (although the estimates are even

[16] Average production wages and population are excluded when examining total manufacturing FDI due to problems with convergence.

[17] As an aside, we often found that we continued to reject the null of homoskedastic errors after performing FGLS using simulated data despite using the correct functional form for the heteroskedasticity and the Klein and Vella (2009) estimator performing well overall.

[18] Standard errors are obtaining using 250 bootstrap repetitions.

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more imprecise) and inclusion of the spatial effects has little influence on the estimated marginal effects of own environmental regulation. Finally, with the appropriate caveats in mind due to the size of the standard errors, it is still interesting to note that the point estimates are smaller in both panels A and B.

In terms of total manufacturing (Table III), the results are consistent with the OLS and Lewbel (2012) approaches, particularly when again considering the size of the standard errors. Thus there is no statistically meaningful evidence of a negative impact of own environmental regulation, or of neighboring environmental regulation, on FDI inflows across the manufacturing sector as a whole; the only exception corresponds to specification 5 when analyzing employment (panel B).

5.3. Sensitivity Analyses

We undertake two final analyses to explore the sensitivity of Lewbel's (2012) and Klein and Vella's (2009) approaches to deviations in the estimation algorithms. In the interest of brevity, we do not report the results.

First, we explore the robustness of Lewbel's (2012) results to the use of a jackknife IV estimator (JIVE) rather than GMM. As shown in Angrist *et al.* (1999) and Chao *et al.* (2012), JIVE has desirable finite sample and asymptotic properties, at least relative to two-stage least squares (TSLS) and limited information maximum likelihood (LIML), particularly in the presence of heteroskedasticity and many instruments. The estimates turn out to be quite imprecise except when FDI is measured by foreign employment in the chemical sector. In this case, own environmental stringency is found to be an economically and statistically significant deterrent to FDI, with magnitudes greater than those found using GMM.

Next, we explore the robustness of Lewbel's (2012) results to alternative instruments. Recall that the baseline results in Table II utilize own (spatially lagged) land values, market proximity, and total road mileage in z_1 (z_2). Here, we explore using each instrument in isolation, as well as augmenting z_1 and z_2 to include own and spatially lagged population, unemployment rate, and unionization rate, respectively. While there is some evidence that the first-stage error variances are related to these variables, it is generally weaker than the variables included in z_1 and z_2 in the baseline model. The results indicate that, while the estimated effects of own environmental regulation are reasonably similar across the various instrument sets, the strength of the instruments and the results of the over-identification tests are less favorable with the new instruments. The weakness of the instruments in these alternative IV sets should not be surprising since our baseline instruments utilized the results of heteroskedasticity tests to determine the most likely candidates to be strong instruments.

Finally, we assess the sensitivity of Klein and Vella's (2009) results to four alternative specifications of the error variances. First, we allow z_1 (and z_2) not only to consist of own (and spatially lagged) land prices, road mileage, market proximity, population, unemployment rate, and unionization rate, but also their squares. Second, we augment z_1 (and z_2) by including the squares as well as interactions of all variables in the baseline model. For the spatial specifications, we do not include interactions between the own and spatial variables. Third, we allow the first- and second-stage error variances to be related to all the exogenous regressors in the model except the state and time fixed effects. Fourth, we include all exogenous regressors and their squares in z_1 (and z_2). The greater flexibility of the functional forms for the error variances leads to some convergence issues. However, the results are qualitatively unchanged.

6. CONCLUSION

The debate over the empirical validity of the PHH is heated and for good reason. To date, however, empirical assessments of the PHH have been hampered by the lack of a credible identification strategy to overcome potential problems associated with measurement error and unobserved heterogeneity. In

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addition, the empirical literature on the PHH has yet to adequately incorporate lessons from the literature on so-called third-country effects. In our view, Kellenberg (2009) comes closest to overcoming these shortcomings, and consequently finds economically and statistically meaningful support for the PHH. Here, we propose two novel identification strategies couched within a model that incorporates spatial effects. Together, the approaches shed new light on the role of environmental regulation in the determination of FDI location.

Specifically, using state-level panel data from 1977 to 1994 from the USA, we consistently find (i) evidence of environmental regulation being endogenous when examining the pollution-intensive chemical sector, (ii) a negative and economically significant impact of own environmental stringency on inbound FDI in the chemical sector, particularly when measured by employment, and (iii) significantly larger effects of environmental regulation on the chemical sector once endogeneity is addressed. The upward bias in standard fixed-effects estimates obtained under the assumption of strict exogeneity is consistent with attenuation bias due to measurement error, as well as important unobservables positively correlated with environmental regulation and FDI inflows (such as tax breaks, investments in other public goods, or agglomeration externalities).

These findings have potentially salient policy implications. Specifically, our results help inform the current debate over environmental federalism (i.e. the appropriate level of government to control environmental policy). As discussed in the survey by Millimet (2013), the canonical model of inter-jurisdictional competition in Oates and Schwab (1988) assumes the perfect mobility of capital, the lack of inter-jurisdictional externalities and social welfare-maximizing governments, among others. In such a framework, decentralized environmental decision making is efficient. However, when these other assumptions of the model fail, capital mobility can lead to inefficient policies. The result is the potential superiority of some form of centralized control over environmental policy. At the country level, this could entail complete federal control over environmental standards (which need not imply uniform standards across sub-jurisdictions) or some form of 'cooperative' federalism whereby the federal government establishes minimum environmental standards and sub-jurisdictions are allowed to exceed this floor if desired (e.g. Esty, 1996). The results here point to the superiority of such a policy arrangement in light of prior evidence suggesting that domestic investment may be even more sensitive to spatial variation in environmental policy than foreign investment (e.g. List *et al.*, 2004).

At the international level, our findings are suggestive of the need for greater centralization as well. However, before reaching important policy conclusions regarding such issues as the WTO's justification to intervene in the domestic environmental policy arena or the sensibility of linking international environmental and trade agreements, further analysis is needed to determine the external validity of the findings obtained here. Our analysis is at the regional level within a single country. Does environmental regulation have similar effects at the country level? Despite this unknown, our results do firmly indicate that policymakers should worry about the incentives for local environmental standards to deviate from Pareto-efficient levels.

Continued research in the future is warranted. First, the prior literature, while suffering from various deficiencies, has emphasized the heterogeneous effects of environmental regulation along numerous dimensions. For instance, Ederington *et al.* (2005) point to substantial heterogeneity across source country (of imports) and the pollution intensity and geographic mobility of the industrial sector. Dean *et al.* (2009) similarly document important heterogeneity by source country (of foreign investment). Henderson and Millimet (2007) and Millimet and List (2004) uncover heterogeneous effects utilizing non-parametric and semi-parametric methods, respectively. Some of this heterogeneity is captured in this study; namely, differential effects by pollution intensity of the sector as well as by measure of FDI (PP&E versus employment). However, other dimensions of heterogeneity uncovered by the prior literature cannot be addressed given the data and identification strategies utilized here. Additional research investigating whether the empirical evidence of heterogeneous effects continues to be present

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once measurement error, spatial effects, and unobserved heterogeneity are accounted for is needed for a deeper understanding of the linkages between environmental and trade policy.

Second, capital mobility related to spatial variation in environmental stringency does not, in and of itself, imply that decentralized environmental policymaking is inefficient. However, in the presence of other 'failures' such as spillovers or rent-seeking behavior by sub-jurisdictional governments, some form of centralized control may be superior. A better connection between the empirical literature on the pollution haven hypothesis and optimal institutional arrangement for environmental policy control is needed.

APPENDIX A: DATA

The BEA regional classification is as follows.

1. New England: Maine, New Hampshire, Vermont, Massachusetts, Rhode Island, Connecticut
2. Mideast: New York, New Jersey, Pennsylvania, Delaware, Maryland
3. Great Lakes: Ohio, Indiana, Illinois, Michigan, Wisconsin
4. Plains: Minnesota, Iowa, Missouri, North Dakota, South Dakota, Nebraska, Kansas
5. Southeast: Georgia, Florida, Virginia, West Virginia, North Carolina, South Carolina, Kentucky, Tennessee, Alabama, Mississippi, Arkansas, Louisiana
6. Southwest: Oklahoma, Texas, Arizona, New Mexico
7. Rocky Mountain: Montana, Idaho, Wyoming, Colorado, Utah
8. Far West: Washington, Oregon, California, Nevada

The Crone (1998/1999) regions—based on a cluster analysis of similar economic activity—are as follows.

1. Maine, New Hampshire, Massachusetts, Arizona, Utah, Montana
2. Ohio, Indiana, Illinois, Michigan, Iowa, Delaware
3. Georgia, Florida, Virginia, North Carolina, South Carolina, Missouri, Kentucky, Tennessee, Alabama, Mississippi, Arkansas, Oklahoma, Rhode Island
4. New York, New Jersey, Pennsylvania, Maryland, Connecticut, West Virginia, Vermont
5. Washington, Oregon, California, Nevada, Idaho, Nebraska, Texas, Wyoming, Minnesota, Louisiana, Kansas
6. North Dakota, South Dakota, Colorado, New Mexico, Wisconsin

APPENDIX B: ESTIMATION ALGORITHMS

B.1. Lewbel's (2012) Approach

Estimation of the empirical model

$$\ln(\text{FDI}_{it}) = X_{it}\Pi + \beta \ln(R_{it}) + \delta \ln\left(\sum_{j\in\Omega} \omega_{ijt} R_{jt}\right) + \varepsilon_{it}$$

proceeds as follows:

1. Regress $\ln(R_{it})$ on X_{it} and obtain $\widehat{\zeta}_{1it}$.
2. Regress $\ln\left(\sum_{j\in\Omega} \omega_{ijt} R_{jt}\right)$ on X_{it} and obtain $\widehat{\zeta}_{2it}$.

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3. Form instruments $\tilde{z}_{rit} \equiv (z_{rit} - \overline{z})\widehat{\zeta}_{rit}$, $r = 1, 2$.
4. Estimate the structural model via GMM using $\tilde{z}_{rit}$, $r = 1, 2$, as instruments for $\ln(R_{it})$ and $\ln\left(\sum_{j\in\Omega} \omega_{ijt} R_{jt}\right)$.

B.2. Klein and Vella's (2009) Approach

Estimation of the empirical model

$$\ln(\text{FDI}_{it}) = X_{it}\Pi + \beta\ln(R_{it}) + \delta\ln\left(\sum\nolimits_{j\in\Omega} \omega_{ijt} R_{jt}\right) + \rho_1 \frac{S_\varepsilon(z_{it})}{S_1(z_{it})}\zeta_{1it} + \rho_2 \frac{S_\varepsilon(z_{it})}{S_2(z_{it})}\zeta_{2it} + \widetilde{\widetilde{\varepsilon}}_{it}$$

proceeds as follows:

1. Regress $\ln(R_{it})$ on X_{it} and obtain $\widehat{\zeta}_{1it}$.
2. Regress $\ln\left(\sum_{j\in\Omega} \omega_{ijt} R_{jt}\right)$ on X_{it} and obtain $\widehat{\zeta}_{2it}$.
3. Estimate θ_j via Poisson pseudo maximum likelihood (PPML) where $\mathrm{E}\left(\widehat{\zeta}^2_{jit}\right) = \exp\left(z_{jit}\theta_j\right)$; compute $\widehat{S}_{jit} = \exp\left(\frac{z_{jit}\widehat{\theta}_j}{2}\right)$, $j = 1, 2$ (see Santos Silva and Tenreyro, 2006).
4. Obtain updated estimates $\widehat{\widehat{\zeta}}_{1it}$ and $\widehat{\widehat{\zeta}}_{2it}$ via feasible generalized least squares (FGLS) using $\widehat{S}_{1it}$ and $\widehat{S}_{2it}$.
5. Verify that the use of $\widehat{S}_{1it}$ and $\widehat{S}_{2it}$ yield homoskedastic errors in the transformed first-stage equations.
6. Obtain updated estimates of θ_j via PPML using $\widehat{\widehat{\zeta}}^2_{jit}$; compute $\widehat{\widehat{S}}_{jit} = \exp\left(\frac{z_{jit}\widehat{\widehat{\theta}}_j}{2}\right)$, $j = 1, 2$.
7. Using $\widehat{\widehat{\zeta}}_{1it}$, $\widehat{\widehat{\zeta}}_{2it}$, $\widehat{\widehat{S}}_{1it}$, and $\widehat{\widehat{S}}_{2it}$, obtain consistent estimates via NLS:

$$\min_{\Pi,\beta,\delta,\rho_1,\rho_2,\theta_\varepsilon} \sum\nolimits_{i,t} \begin{bmatrix} \ln(\text{FDI}_{it}) - X_{it}\Pi - \beta\ln(R_{it}) - \delta\ln\left(\sum\nolimits_{j\in\Omega}\omega_{ijt}R_{jt}\right) \\ -\rho_1\sqrt{\exp\left(z_{\varepsilon it}\theta_\varepsilon\right)}\left(\frac{\widehat{\widehat{\zeta}}_{1it}}{\widehat{\widehat{S}}_{1it}}\right) - \rho_2\sqrt{\exp\left(z_{\varepsilon it}\theta_\varepsilon\right)}\left(\frac{\widehat{\widehat{\zeta}}_{2it}}{\widehat{\widehat{S}}_{2it}}\right) \end{bmatrix}^2$$

8. Estimate θ_ε (again) via PPML where $\mathrm{E}\left(\widehat{\varepsilon}^2_{it}\right) = \exp\left(z_{it}\theta_\varepsilon\right)$, where

$$\widehat{\varepsilon}_{it} = \ln(\text{FDI}_{it}) - X_{it}\widehat{\Pi} - \widehat{\beta}\ln(R_{it}) - \widehat{\delta}\ln\left(\sum\nolimits_{j\in\Omega}\omega_{ijt}R_{jt}\right)$$

and compute $\widehat{S}_{\varepsilon it} = \exp\left(\frac{z_{\varepsilon it}\widehat{\theta}_\varepsilon}{2}\right)$.

9. Use $\widehat{S}_{\varepsilon it}$ to estimate via FGLS:

$$\ln(\text{FDI}_{it}) = X_{it}\Pi + \beta\ln(R_{it}) + \delta\ln\left(\sum\nolimits_{j\in\Omega}\omega_{ijt}R_{jt}\right) + \underbrace{\rho_1\left(\frac{\widehat{S}_{\varepsilon it}}{\widehat{\widehat{S}}_{1it}}\widehat{\widehat{\zeta}}_{1it}\right) + \rho_2\left(\frac{\widehat{S}_{\varepsilon it}}{\widehat{\widehat{S}}_{2it}}\widehat{\widehat{\zeta}}_{2it}\right)}_{\text{control function}} + \widetilde{\widetilde{\varepsilon}}_{it}$$

10. Compute standard errors via bootstrap.

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