Three New Empirical Tests of the Pollution Haven Hypothesis When Environmental Regulation is Endogenous

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May 7, 2013

Abstract

The Pollution Haven Hypothesis (PHH) refers to the claim that production within polluting industries will shift to locations with lax evironmental regulation. While straightforward, empirical analysis of the PHH has been anything but straightforward. The existing literature is inconclusive due to two shortcomings. First, the issues of unobserved heterogeneity and measurement error in environmental regulation are typically ignored due to the lack of a credible, traditional instrumental variable. Second, while the recent literature has emphasized the importance of geographic spillovers in determining the location choice of foreign investment, such spatial effects have yet to be adequately incorporated into empirical tests of the PHH. As a result, the impact of environmental regulations on trade patterns and the location decisions of multinational enterprises remains unclear. In this paper, we circumvent the lack of a traditional instrument within a model incorporating geographic spillovers utilizing three novel identification strategies. Using state-level panel data on U.S. inbound foreign direct investment (FDI), relative abatement costs, and other determinants of FDI, we consistently find (i) evidence of environmental regulation being endogenous, (ii) a negative impact of own environmental regulation on inbound FDI in pollution-intensive sectors, particularly when measured by employment, and (iii) larger effects of environmental regulation once endogeneity is addressed. Neighboring environmental regulation is not found to be an important determinant of FDI.

JEL: C31, F23, Q52
Keywords: Foreign Direct Investment, Environmental Regulation, Spillovers, Instrumental Variables, Control Function, Heteroskedasticity

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*The authors are grateful to Arik Levinson for providing the data, and comments from Ed Vytlacil, three anonymous referees, Arik Levinson, Arthur Lewbel, Frank Vella, Per Fredriksson, seminar participants at the Dallas Federal Reserve Bank, Louisiana State University, University of Louisville, SMU, and Tulane University, and conference participants at Texas Camp Econometrics XVI, the 2010 Southern Economic Association Meetings, the 2011 Western Economic Association Meetings, and the 2012 NBER EEE Summer Meetings. Corresponding author: Daniel Millimet, Department of Economics, Box 0496, Southern Methodist University, Dallas, TX 75275-0496. Tel: (214) 768-3269. Fax: (214) 768-1821. E-mail: millimet@smu.edu.

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 U.S. – 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 during the Great Recession, the U.S. 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. 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."

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; Ekholm et al. 2003; Grossman et al. 2003; 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 U.S. 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 U.S. 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 three novel identification strategies designed to circumvent the need to identify valid exclusion restrictions in the usual sense.

The first and second estimation 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. In light of the similarity between these two approaches, as well as the nonstandard approach to identification, we undertake a third identification approach that is a bit more transparent as a robustness exercise. The third method generates instruments utilizing a differencing strategy based on Pitt and Rosenzweig (1990). The key identifying assumptions are that the marginal effects of certain covariates are identical across pollution-intensive and non-pollution-intensive sectors, and these covariates are significantly related to environmental stringency.

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 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)

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

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 1. At the risk of over-simplifying 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 non-linearities (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 overidentification 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.

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

To fix ideas, a typical model used to assess determinants of (continuous) measures of FDI stocks or flows with panel data is a standard two-way fixed effects specification:

$$\ln(FDI_{it}) = \eta_i + \lambda_t + \textstyle\sum_k \beta_k x_{ikt} + \varphi_{it}, \tag{1}$$

where FDI is some measure of MNE activity in location i and time t, x_k, $k = 1, ..., K$, are time-varying observable attributes of location i, η_i and λ_t are location and period fixed effects, respectively, and φ_{it} is the error term. A proxy for environmental regulation is one element in x. Strict exogeneity of x is required for consistency.

As discussed previously, the model in (1) is potentially flawed due to the exclusion of geographic spillovers. The omission of spillovers is one reason why the strict exogeneity assumption may fail in practice. Thus, we begin by augmenting (1) to include spatially lagged counterparts for each covariate:

$$\ln(FDI_{it}) = \eta_i + \lambda_t + \textstyle\sum_k \left[\beta_k x_{ikt} + \delta_k \sum_{j\in\Omega} \omega_{ijt} x_{jkt}\right] + \varepsilon_{it}, \tag{2}$$

where $\varepsilon_{it} \equiv \varphi_{it} - \sum_k \delta_k \sum_{j\in\Omega} \omega_{ijt} x_{jkt}$, ω_{ijt} is the weight given by location i to neighbor j in period t, Ω includes the set of neighbors of location i, and ε_{it} is the new error term.[2] Even if all elements in the regressors in the augmented model are strictly exogenous, estimation of (2) is nonstandard given the introduction of the weights, ω.

To proceed, the weights must be chosen *a priori* and this choice is necessarily ad hoc.[3] Because the true weights are unknown, we utilize four straightforward weighting schemes. First, we assign a weight of zero to non-contiguous

[2] One might consider augmenting the model in (2) 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 (2). Similarly, one might consider augmenting (2) 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, e.g., 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.

[3] 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

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

where x is the covariate and ω^*_{ij} is the 'true' weight placed on state j by state i. If the weights are mis-specified such that the assumed 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 U.S. 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 U.S. Bureau of Economic Analysis (BEA) and Crone (1998/1999). The BEA regional classification system was introduced in the 1950s 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 U.S. 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 U.S. 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 (2) 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 first two approaches to identification, we re-write (2) more compactly, as well as introduce the first-stage equations, in order to make explicit the system of equations we are estimating. The system of equations is given by

$$\begin{aligned}
\ln(FDI_{it}) &= X_{it}\Pi + \beta\ln(R_{it}) + \delta\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}) + \varepsilon_{it} && (3)\\
\ln(R_{it}) &= X_{it}\Pi_R + \zeta_{1it} && (4)\\
\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}) &= X_{it}\Pi_{SR} + \zeta_{2it}, && (5)
\end{aligned}$$

where R is the proxy for environmental regulation, X includes all the other regressors from x in (2) 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.[4] All errors are assumed to be mean zero. Note, the model is not identified in the traditional sense since there are no exclusion restrictions in (4) and (5).

weight is

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

then substitution yields

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

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, $\psi_{ij} \neq 0$ generates heteroskedasticity which is actually exploited for identification by the estimators used in this paper. If ψ and x are not indepdendent, then OLS estimates of δ will be biased in a nontrivial 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.

[4] The log-linear specification of (3) follows from a theoretical gravity model for FDI (e.g., Kleinert and Toubal 2010; Schmeiser 2013).

3.2 Lewbel (2012) Approach

The Lewbel (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.

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

$$\mathrm{E}[z_r'\zeta_r^2] \neq 0 \tag{6}$$

$$\mathrm{E}[z_r'\varepsilon\zeta_r] = 0 \tag{7}$$

for $r = 1, 2$. If these assumptions are satisfied, then $\widetilde{z}_r \equiv (z_r - \overline{z})\zeta_r$, $r = 1, 2$, are valid instruments. For instance, if the errors in (3) – (5) 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 re-write the errors in (3) – (5) as

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

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 (6) and (7) are satisfied. Note, $\widetilde{\varepsilon}$ 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) is drawn from an identical distribution across observations. However, the idiosyncratic shocks to environmental stringency may be drawn from different distributions.[5]

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

[5] Note, measurement error in the weights does not, in general, satisfy these assumptions. In the simplified model given in footnote 3, κ_i is equal to $-\sum_{j\in\Omega}\psi_{ij}x_j$ which is heteroskedastic with variance depending on x. Thus, setting $z = x$ would not satisfy the restricition in (7). However, if we extend this simplified model to allow for two covariates, as in

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

then substitution yields

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

In this case, if, say, $\delta_1 = 0$, then x_1 may serve the role of z in order to derive an instrument for, say, x_2, 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 $\delta_2\sum_{j\in\Omega}\psi_{ij}x_{2j}$. Moreover, homoskedastic measurement error in the covariates themselves (as opposed to measurement error in the weights) would also satisfy (6) and (7) as long as the variances of the idiosyncratic errors depend on x.

counterparts of these variables are included in z_2 (discussed below). The instruments, $\widetilde{z}_r$, 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, 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 & Vella (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 (3) – (5). However, rather than invoking the assumptions given in (6) and (7) concerning the errors, the following assumptions are made:

$$\varepsilon_{it} = S_\varepsilon(z_{it})\varepsilon^*_{it} \tag{8}$$

$$\zeta_{rit} = S_r(z_{it})\zeta^*_{rit}, \quad r = 1, 2 \tag{9}$$

$$S_\varepsilon(z_{it})/S_r(z_{it}), \quad r = 1, 2, \text{ varies across } i \tag{10}$$

$$\mathrm{E}[\varepsilon^*_{it}\zeta^*_{rit}] = \rho_r, \quad r = 1, 2 \tag{11}$$

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. However, the conditional correlation, ρ_r, $r = 1, 2$, between the underlying homoskedastic portion of the errors must be fixed. Note, 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

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

where $\widetilde{\varepsilon}$ and $\widetilde{\zeta}_r$ are mean-zero, independent of X and κ, and have a constant correlation given by ρ_r, then (8) – (11) are satisfied.

Referring back to (11), 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 2). Agglomeration may affect environmental stringency due to the scale effect of pollution-generating activity. However, the impact may depend on state-level attributes, z_{it}. For instance, states with attributes that are not conducive to attracting FDI may limit the impact of agglomeration on environmental stringency. Similarly, own agglomeration may impact FDI through economies of scale, but the effect may depend on state-level attributes as well. Neighboring agglomeration may adversely impact FDI by improving the desirability of neighboring locations. However, once we condition on these state-level attributes, the return to own and neighboring agglomeration, ρ_1 and ρ_2, respectively, are constant. While not testable, this seems plausible.

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

$$S_\varepsilon(z_{it}) = \exp\left(\frac{z_{\varepsilon it}\theta_\varepsilon}{2}\right) \tag{12}$$

$$S_r(z_{it}) = \exp\left(\frac{z_{rit}\theta_r}{2}\right), \quad r = 1,2 \tag{13}$$

where z_r includes additional covariates beyond those employed in the Lewbel (2012) approach.[6] 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 setup, (3) may be re-written as

$$\ln(FDI_{it}) = X_{it}\Pi + \beta\ln(R_{it}) + \delta\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}) + \rho_1\frac{S_\varepsilon(z_{it})}{S_1(z_{it})}\zeta_{1it} + \rho_2\frac{S_\varepsilon(z_{it})}{S_2(z_{it})}\zeta_{2it} + \tilde{\tilde{\varepsilon}}_{it} \tag{14}$$

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 $\tilde{\tilde{\varepsilon}}_{it}$ is a well-behaved error term. Given the functional form assumptions in (12) and (13), (14) 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.[7] Summary statistics are provided in Appendix A. The data cover the 48 contiguous U.S. states from 1977 – 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).[8,9] 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

[6] The Lewbel (2012) approach does not require one to identify *all* covariates satisfying (6) and (7). All we require is a sufficient number of (valid) instruments to identify the model. In fact, too many instruments may have undersirable effects particularly if some instruments are weak (Wooldridge 2002). However, the Klein and Vella (2009) approach requires a consistent estimate of $S_r(z_{it})$, $r = 1,2$.

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

[8] 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).

[9] 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.

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; 190,000 to 500,000 in the chemical sector.

In the theoretical model of inbound FDI presented in Blonigen et al. (2008), 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.[10] 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 U.S., time effects capture parent country attributes. All variables are expressed in logarithmic form with the exception of the unemployment and unionization rates. In addition, 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 experiencing the largest decline in RAC. In addition, Table A1 in the 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.

[10] 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.

5 Results

5.1 Lewbel (2012) Approach

The baseline results are presented in Tables 2 and 3. Table 2 contains the results for the chemical sector only; Table 3 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 the Lewbel (2012) approach are given under the column labelled 'IV'. OLS estimates are presented for comparison, where the Specification 1 results are identical to Keller and Levinson (2002).[11]

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.[12] 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 3). In addition, the OLS estimates are fairly stable across the five specifications; neighboring environmental regulation is statistically significant only in Specifications 2 and 3 when assessing employment in the chemical sector (Panel B, Table 2). 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

[11] We only display the point estimates for own and neighboring environmental regulation to conserve space. Full estimation results are available upon request. However, Tables A2 and A3 in Appendix A 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).

[12] 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 4. Additional results – using other weighting schemes or for other covariates – are available upon request.

included. Specifically, we reject the null that the model is underidentified at the $p < 0.01$ confidence level in every case using 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 four cases at the $p < 0.10$ confidence level for Specifications 1, 3, 4, and 5. Thus, the Lewbel (2012) approach performs well. 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 2), the GMM estimates are statistically significant at at least 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. The fact that the IV estimates suggest a stronger adverse effect of environmental regulation is consistent with many of the papers listed in Table 1, 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 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 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 3), 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 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 & Vella (2009) Approach

The results from the Klein and Vella (2009) approach are also presented in Tables 2 and 3 under the column labelled 'CF' (for control function). As noted above, we include an expanded set of variables in z_1 and z_2 relative to the Lewbel (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.[13]

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 4, 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.[14]

Turning to the results for the chemical sector in Table 2, the point estimates for own environmental regulation are fairly stable across the five specifications, particularly in Panel A (PP&E).[15] 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 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 larger in absolute value

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

[14] 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.

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

relative to the Lewbel (2012) estimates when using PP&E (Panel A) to measure FDI, but generally smaller when using employment (Panel B) to measure FDI.

In terms of total manufacturing (Table 3), 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 several further analyses to explore the sensitivity of our results. First, we implement the Lewbel (2012) using 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. Second, we explore the robustness of the Lewbel (2012) results to alternative instrument sets. Finally, we utilize a third estimation approach designed to generate exclusion restrictions when traditional instruments are unavailable. Based on the approach put forth in Pitt and Rosenzweig (1990), this estimator does not involve restrictions on higher moments. Instead, this approach achieves identification under a completely unrelated set of restrictions.

5.3.1 JIVE

Table 5 presents the Lewbel (2012) results obtained using the JIVE1 estimator of Angrist et al. (1999). This estimator entails obtaining (out-of-sample) fitted values of the endogenous regressors for each state-time observation after estimation of (4) and (5) omitting one observation at a time. After estimation of the fitted values, these are used as instruments in the usual instrumental variable estimator. The estimates are quite imprecise except when FDI is measured in terms of foreign employment in the chemical sector. In this case, own environmental stringency is found to be an economically and statistically significant deterrent to FDI. In addition, the magnitude of the effect is larger than the GMM estimates reported in Table 2. To put the magnitude of the effects in context, let us return to our prior thought experiment and use the point estimate from Specification 5. If Ohio in 1991 had increased its RAC at the time to match California, employment in foreign-owned affiliates in the chemical sector would have fallen from 17,600 to roughly 14,200 in expectation; the GMM predicted decline from above is 15,600.

5.3.2 Alternative Instruments

Table 6 presents select results obtained using alternative instruments in the Lewbel (2012) approach. Specifically, we report results obtained using Specifications 1 (no spatial lags) and 5 (spatial lags using distance weights), where the instruments are based on different sets of variables in z_1 and z_2. Recall, the baseline results in Table 2 (repeated in Table 5 for convenience) utilize own (spatially lagged) land values, market proximity, and total road mileage in z_1 (z_2). Here, IV Set A – C utilize each of these variables individually. IV Set A uses own and spatially lagged land values in z_1 and z_2, respectively. IV Set B (C) uses own and spatially lagged market proximity (total road mileage). IV Set D uses own (spatially lagged) population, unemployment rate, and unionization rate in z_1 (z_2). While there is some evidence that the first-stage error variances are related to these variables, it is generally weaker than the variables we utilize in the baseline model. Finally, IV Set E combines the variables used in the baseline model with the variables used in IV Set D. Note, the models using IV Sets A – C are exactly identified.

The results indicate that while the estimated effects of own environmental regulation are reasonably similar across the various instrument sets, particularly in Specification 1, the strength of the instruments and the results of the overidentification tests are less favorable with the new instruments. The weakness of the instruments in these alternative IV sets should not be surprising. First, as stated above, our baseline instruments utilized the results of heteroskedasticity tests to determine the most likely candidates to be strong instruments. Second, the poor performance of IV Sets A – C in Specification 5, relative to the baseline model, is attributable to the fact that while one variable, say own market proximity, may be related to the error variance in (4), its spatial lag may be weakly related to the error variance in (5). Thus, the set of instruments utilized in the baseline model gives us strong identification, despite the weak identification using IV Sets A – C in Specification 5.

5.3.3 Pitt & Rosenzweig (1990) Approach

Our final sensitivity analysis turns to a completely different approach to identification than the estimators considered up to this point. The approach is borrowed from Pitt and Rosenzweig (1990), who are concerned with the impact of an endogenous household-level variable on children differentiated by gender. Lacking a traditional exclusion restriction, the solution proposed entails examining the differential effect of the endogenous variable on sons versus daughters and generating exclusion restrictions by assuming that some exogenous household-level covariates have identical effects on boys and girls. In our application, we apply this logic to assess the differential effect of own and spatially lagged environmental regulation on two types of FDI: FDI in pollution-intensive and non-pollution-intensive sectors. Valid exclusion restrictions are generated by assuming that some exogenous state-level covariates have equal effects on FDI across these two sectors.

Formally, we re-write the second-stage equation in (3) separately for pollution-intensive (P) manufacturing sectors and non-pollution-intensive (NP) manufacturing sectors:

$$\begin{aligned} \ln(FDI_{it}^{p}) &= X_{1it}\Pi_1^{p} + X_{2it}\Pi_2 + \beta^{p}\ln(R_{it}) + \delta^{p}\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}) + \varepsilon_{it}^{p} && (15) \\ \ln(FDI_{it}^{np}) &= X_{1it}\Pi_1^{np} + X_{2it}\Pi_2 + \beta^{np}\ln(R_{it}) + \delta^{np}\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}) + \varepsilon_{it}^{np}, && (16) \end{aligned}$$

where $X = [X_1\ X_2]$ and Π_1 and Π_2 are conformable vectors of parameters. Thus, X_1 includes a subset of X for which the marginal effects on FDI are allowed to differ depending on the pollution intensity of the industry. The marginal effects of covariates included in X_2 are assumed to be constant across sectors (and thus lack a p or np superscript). In the analysis, we take the chemical sector as the pollution-intensive sector and all other manufacturing sectors as the (relatively) non-pollution-intensive sector (e.g., Xing and Kolstad 2002; Wagner and Timmins 2009). While there are certainly other data partitions one may try (conditional on data availability), results based on treating only the chemical sector as pollution-intensive should yield evidence of the PHH if it exists given that studies such as Copeland and Taylor (2004, p. 45) consider the industry to be "one of the dirtiest."

Subtracting (16) from (15) in each time period yields

$$\Delta \ln(FDI_{it}) = \ln(FDI_{it}^{p}) - \ln(FDI_{it}^{np}) = X_{1it} \cdot \Delta\Pi_1 + \Delta\beta \cdot \ln(R_{it}) + \Delta\delta \cdot \ln(\textstyle\sum_{j\in\Omega} \omega_{ijt} R_{jt}) + \Delta\varepsilon_{it}. \tag{17}$$

Thus, Δ represents the difference between P and NP at a point in time, not the change over time. In (17), X_2 is available as an exclusion restriction.[16] Thus, the first-stage equations are given by

$$\ln(R_{it}) = X_{1it}\Pi_{R1} + X_{2it}\Pi_{R2} + \zeta_{1it} \tag{18}$$

$$\ln(\textstyle\sum_{j\in\Omega} \omega_{ijt} R_{jt}) = X_{1it}\Pi_{SR1} + X_{2it}\Pi_{SR2} + \zeta_{2it} \tag{19}$$

which are, in fact, identical to (4) and (5).

This identification strategy relies on choosing a set of exogenous controls, X_2, and imposing the restrictions that $\Pi_2^p = \Pi_2^{np} = \Pi_2$. If the restrictions imposed lead to an overidentified model (i.e., there are at least two variables in X_2), then the usual overidentification test constitutes a test of the restrictions imposed. To see this, note that if Π_2 does in fact differ across sectors differentiated by pollution intensity, then the error term in (17) will contain $X_{2it} \cdot \Delta\Pi_2$ and the resulting instruments will be invalid.

While this approach aids in the generation of instruments for environmental regulation, it does so at a cost (even if the restrictions are valid). Specifically, the approach only provides consistent estimates of $\Delta\Pi_1$, $\Delta\beta$, and $\Delta\delta$, not the individual, structural parameters from (16) and (15). Thus, ignoring the case where Π_1^{np}, β^{np}, or δ^{np} equal zero, this approach only identifies the differential effects of X_1, own environmental regulation, and neighboring environmental regulation on inbound FDI in pollution-intensive relative to non-pollution-intensive sectors. While one might be tempted to assume that the parameters in β^{np} and δ^{np} corresponding to own and spatially lagged environmental regulation are zero, this is unlikely to be the case. On the one hand, our definition of the non-pollution-intensive sector still includes some polluting industries since it aggregates all manufacturing sectors except the chemical sector. On the other hand, when treating environmental regulation as exogenous and ignoring geographic spillovers, Henderson and Millimet (2007) obtain some positive and statistically meaningful nonparametric regression estimates of the association between environmental regulation and inbound U.S. FDI. Similarly, Mulatu et al. (2010) obtain a statistically insignificant, but positive effect of environmental stringency on production shares in the least pollution-intensive sectors. Thus, the magnitudes and even the signs of β^{np} and δ^{np} are unknown.

The results are presented in Table 7. Estimation is by GMM, where the variables in X_2 include demand-side variables – market proximity and population – and tax effort as such variables should be equally relevant to all manufacturing industries.[17] On the contrary, abatement costs, unemployment and unionization rates, average production-worker wages, land prices, and energy prices are likely to have a differential effect on pollution-intensive

[16] Note, this approach is similar to the strategy employed in Wagner and Timmins (2009). However, in that study, the authors assume that environmental regulation is exogenous in their analog to the differenced equation given by (17). Thus, their identification strategy requires no location-specific unobservables be correlated with environmental regulation, but differentially associated with investment across sectors differentiated by pollution intensity. As shown below, this assumption is rejected in our data.

[17] As in the Lewbel (2012) approach, heteroskedasticity-robust standard errors are used (Baum et al. 2007).

and non-pollution-intensive FDI given that polluting industries are expected to be capital-, energy-, and land-intensive while non-polluting industries are likely to be labor-intensive (Mani and Wheeler 1998). Accordingly, these variables are included in X_1. Similarly, the inclusion of road mileage in X_1 is also reasonable given the chemical sector's reliance on road transport (Chen and Nunez 2006).[18,19] In addition, region and time fixed effects are included in X_1. The inclusion of region effects, rather than state effects, restricts time invariant attributes that differentially affect sectors according to their pollution intensity are constant within regions.

Turning to the results, four findings emerge. First, the OLS estimates are negative and statistically significant, as well as significantly larger in absolute value than the OLS estimates reported in Tables 2 and 3. This difference follows from a change in the definition of the dependent variable, as well as the fact that the model in (17) eliminates time-varying unobservables that are correlated with both FDI and environmental regulation, but affect FDI equally across sectors (e.g., local macroeconomic shocks or political corruption). In addition, the OLS estimates are fairly stable across the five specifications; neighboring environmental regulation is rarely statistically significant.

Second, the identification strategy works well as determined by the usual IV specification tests, particularly when using employment to measure FDI. Specifically, we reject the null that the model is underidentified at the $p < 0.01$ confidence level in every case using Kleibergen and Paap's (2006) rk statistic, and the Kleibergen and Paap (2006) F-statistic is reasonably large in most cases. In addition, we fail to reject the validity of the instruments using Hansen's J-test in all cases when using employment to measure FDI, and in Specifications 2 and 3 in Panel A at the $p < 0.05$ confidence level. Third, we reject exogeneity of environmental regulation in all cases.

Finally, the GMM estimates are statistically significant at the $p < 0.01$ confidence level using either the traditional approach or the Anderson and Rubin (1949) test robust to weak instruments in all cases. Moreover, the point estimates are considerably larger in magnitude than the OLS estimates in absolute value; however, the standard errors are also roughly four to six times larger. Neighboring environmental regulation is rarely statistically significant although the estimates are very imprecise.

While the point estimates for own environmental regulation are economically large, they are very similar in magnitude to the IV estimates obtained in Kellenberg (2009).[20] To put the magnitude in context, let us return to our prior thought experiment and use the point estimate from Specification 5. Ohio in 1991 had 17,600 (104,900) workers in foreign-owned affiliates in the chemical sector (all other manufacturing sectors); thus, relative employment in the chemical sector was 16.8%. The *ceteris paribus* effect of Ohio increasing its RAC at the time to match California would have been a decline in relative employment in the chemical sector to 12.3%. In contrast, the OLS estimate implies a decline to only 15.6%. What is not known is how much of this change is due to a relative decline in employment in the pollution-intensive sector and how much is due to a relative increase in employment in the non-pollution-intensive sector. As stated previously, Henderson and Millimet (2007) find heterogeneous, and often

[18] Also, according to the 2007 Commodity Flow Survey, the chemical manufacturing industry transported nearly 40% of its hazardous materials tonnage by truck. See http://www.bts.gov/publications/special_reports_and_issue_briefs/special_report/2011_01_26/html/entire.html.

[19] As an alternative, we also treated tax effort as the only regressor in X_2, thereby yielding exactly identified models. However, the instruments proved to be very weak.

[20] Kellenberg (2009) obtains elasticities in the -2 to -3 range. When he focuses exclusively on a measure of environmental enforcement to capture regulatory costs, the elasticity approaches -5.

positive, associations between own environmental regulation and total manufacturing FDI when treating regulation as exogenous.

6 Conclusion

The debate over the empirical validity of the PHH is heated and for good reason; the answer has far-reaching consequences at the local, national, and international levels. 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 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 three novel identification strategies couched within a model that incorporates spatial effects. Together, the three approaches shed new light on the role of environmental regulation in the determination of FDI location.

Specifically, using state-level panel data from 1977-1994 from the U.S., 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).

While informative, continued research is warranted. First, the analysis here is at the regional level. 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. 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. Such fears are particularly worrisome since prior evidence suggests that domestic investment may be even more sensitive to spatial variation in environmental policy than foreign investment (e.g., List et al. 2004).

Second, 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 nonparametric and semiparametric 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. Future research investigating whether the empirical evidence of heterogeneous effects continues to be present 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.

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A Data Appendix

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

Table A1. Summary Statistics

Variable	Full Sample			RAC ≤ 1			RAC > 1		
	N	Mean	SD	N	Mean	SD	N	Mean	SD
Total Manufacturing FDI (PP&E)	811	2859.31	3919.40	449	2796.21	3610.31	362	2937.57	4275.67
Chemical Sector FDI (PP&E)	563	1016.94	1732.51	323	812.53	1117.36	240	1292.04	2289.90
Non-Chemical Sector FDI (PP&E)	563	1491.44	1923.02	323	1440.59	1703.17	240	1559.88	2186.17
Total Manufacturing FDI (Employment)	814	32680.75	36602.64	452	37596.70	39038.80	362	26542.60	32329.69
Chemical Sector FDI (Employment)	621	7691.73	9641.22	349	8760.95	10820.55	272	6319.83	7677.71
Non-Chemical Sector FDI (Employment)	621	23377.44	26783.65	349	26047.92	26573.23	272	19950.99	26710.69
Relative Abatement Costs (RAC)	816	1.02	0.37	452	0.76	0.16	364	1.34	0.31
Unemployment Rate	816	6.61	2.09	452	6.14	1.87	364	7.20	2.21
Agricultural Land Values ($/per acre)	816	887.02	775.04	452	974.93	896.84	364	777.84	572.50
Energy Prices, Industrial Sector	816	5.51	1.70	452	5.89	1.85	364	5.04	1.35
Highway Road Mileage	816	80500.90	48367.55	452	82200.68	42205.87	364	78390.18	55056.13
Population (millions)	816	4.94	5.13	452	5.45	5.43	364	4.30	4.67
Unionization Rate	816	16.55	6.71	452	17.37	6.86	364	15.54	6.38
Average Production Worker Wages, Manufacturing Sector ($/hr)	816	9.10	2.24	452	9.09	2.28	364	9.10	2.19
Tax Effort	816	96.06	16.05	452	98.51	18.89	364	93.01	10.88
Market Proximity	816	6630.94	8220.03	452	8218.10	9694.03	364	4660.08	5283.64

Table A2. Select Full Results for Chemical Sector PP&E: Lewbel (2012) and Klein & Vella (2009) Approaches.

	Lewbel (2012)						Klein & Vella (2009)					
	Spec (1)		Spec (3)		Spec (5)		Spec (1)		Spec (3)		Spec (5)	
	Own X	Spatial X	Own X	Spatial X	Own X	Spatial X	Own X	Spatial X	Own X	Spatial X	Own X	Spatial X
ln(RAC)	-0.567*		-0.547*	-1.129†	-0.432†	-1.356	-0.404		-0.375	-0.673	-0.334	1.213
	(0.214)		(0.202)	(0.469)	(0.208)	(1.005)	(0.454)		(0.318)	(0.525)	(0.350)	(1.547)
ln(Wages)	-1.034		-1.189	-1.487	-0.341	-6.736	-0.18		-0.191	1.314	0.478	-3.698
	(0.807)		(0.863)	(1.577)	(0.878)	(4.307)	(1.438)		(1.559)	(2.719)	(1.843)	(5.992)
ln(Land	-0.341†		-0.409†	0.577‡	-0.317‡	1.396‡	-(0.352)		-(0.448)	(0.019)	-(0.381)	-(0.301)
Values)	(0.135)		(0.183)	(0.300)	(0.192)	(0.750)	(0.271)		(0.307)	(0.540)	(0.344)	(1.260)
ln(Energy	0.048		0.160	0.007	0.087	-0.257	-0.052		0.196	0.055	-0.093	0.167
Prices)	(0.235)		(0.237)	(0.392)	(0.259)	(1.003)	(0.412)		(0.422)	(0.507)	(0.486)	(1.681)
ln(Tax	-0.213		-0.219	-0.823	-0.287	1.727	-0.134		-0.090	-1.300	-0.210	0.071
Effort)	(0.302)		(0.296)	(0.595)	(0.304)	(1.576)	(0.546)		(0.550)	(0.947)	(0.553)	(2.731)
ln(Market	1.635*		2.382*	-1.657†	1.919*	-1.044	(01.126)		1.998‡	-(02.095)	1.392	2.565
Proximity)	(0.451)		(0.528)	(0.698)	(0.538)	(2.322)	(0.742)		(1.077)	(1.276)	(1.016)	(5.347)
ln(Pop)	-0.922		-3.253*	3.482†	-2.533†	5.768‡	-0.625		-3.07	4.844‡	-1.888	4.491
	(0.653)		(0.897)	(1.352)	(1.083)	(3.224)	(1.404)		(2.211)	(2.540)	(2.385)	(7.325)
Unemplymt	0.043*		0.050*	-0.002	0.063*	-0.029	0.02		0.036	-0.021	0.036	0.021
Rate	(0.017)		(0.019)	(0.032)	(0.018)	(0.065)	(0.029)		(0.030)	(0.039)	(0.033)	(0.109)
Unionization	-0.110*		-0.106*	-0.006	-0.114*	-0.191†	-0.099*		-0.104*	-0.007	-0.103*	-0.163
Rate	(0.013)		(0.013)	(0.034)	(0.014)	(0.078)	(0.025)		(0.027)	(0.062)	(0.031)	(0.177)
ln(Road	-0.954‡		-0.960	-0.120	-0.421	-0.243	-1.372		-1.528	-0.347	-0.797	-1.410
Mileage)	(0.532)		(0.675)	(0.925)	(0.644)	(2.076)	(1.120)		(1.239)	(1.298)	(1.245)	(4.600)
N	563		563		563		563		563		563	

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors are in parentheses; Lewbel (2012) standard errors are heteroskedasticity-robust; Klein & Vella (2009) standard errors are obtained via bootstrap. See Table 2 for further details.

Table A3. Select Full Results for Chemical Sector Employment: Lewbel (2012) and Klein & Vella (2009) Approaches.

	Lewbel (2012)						Klein & Vella (2009)					
	Spec (1)		Spec (3)		Spec (5)		Spec (1)		Spec (3)		Spec (5)	
	Own X	**Spatial X**	**Own X**	**Spatial X**	**Own X**	**Spatial X**	**Own X**	**Spatial X**	**Own X**	**Spatial X**	**Own X**	**Spatial X**
ln(RAC)	-0.836*		-0.673*	-0.809*	-0.695*	-1.133	-0.468		-0.619‡	-1.380‡	-0.267	0.452
	(0.161)		(0.141)	(0.297)	(0.162)	(0.853)	(0.572)		(0.359)	(0.749)	(0.316)	(1.095)
ln(Wages)	0.35		0.092	-0.668	1.301‡	-4.203	-0.517		-0.064	-0.475	1	-6.708
	(0.675)		(0.640)	(1.061)	(0.724)	(3.762)	(1.280)		(1.451)	(2.115)	(1.383)	(6.951)
ln(Land	-(0.067)		-(0.058)	-(0.112)	(0.024)	(0.381)	-(0.095)		-0.449‡	(0.365)	-(0.398)	(0.175)
Values)	(0.111)		(0.136)	(0.228)	(0.144)	(0.644)	(0.229)		(0.247)	(0.434)	(0.265)	(1.037)
ln(Energy	-0.035		0.066	0.205	-0.116	0.435	-0.236		-0.247	0.063	-0.267	0.461
Prices)	(0.187)		(0.172)	(0.288)	(0.195)	(0.812)	(0.323)		(0.315)	(0.471)	(0.344)	(0.954)
ln(Tax	0.037		-0.034	-1.714*	-0.009	4.144*	0.002		-0.088	-0.994	-0.071	2.445
Effort)	(0.277)		(0.259)	(0.461)	(0.271)	(1.302)	(0.519)		(0.480)	(0.849)	(0.421)	(2.152)
ln(Market	1.052*		1.725*	-(0.599)	1.479*	4.650†	1.296†		1.451‡	(0.529)	1.129	7.749
Proximity)	(0.381)		(0.399)	(0.579)	(0.425)	(1.907)	(0.641)		(0.850)	(1.179)	(0.688)	(4.893)
ln(Pop)	-1.398*		-3.701*	3.929*	-3.326*	2.661	0.046		-0.46	-0.267	-0.399	-1.582
	(0.502)		(0.590)	(0.933)	(0.702)	(2.360)	(1.191)		(1.891)	(2.078)	(1.363)	(4.853)
Unemplymt	0.062*		0.059*	0.036	0.079*	-0.006	0.04		0.058‡	0.071	0.058‡	0.045
Rate	(0.016)		(0.016)	(0.025)	(0.018)	(0.058)	(0.034)		(0.032)	(0.045)	(0.033)	(0.093)
Unionization	-0.025†		-0.029*	0.003	-0.024†	-0.074	-0.025		-0.042‡	-0.013	-0.033	-0.073
Rate	(0.012)		(0.011)	(0.027)	(0.012)	(0.069)	(0.030)		(0.025)	(0.055)	(0.029)	(0.132)
ln(Road	-0.945†		-0.785†	0.067	-0.556	-0.641	0.246		-1.086	-2.077	-0.380	2.181
Mileage)	(0.433)		(0.392)	(0.703)	(0.463)	(1.717)	(0.740)		(0.881)	(1.643)	(0.812)	(4.172)
N	621		621		621		621		621		621	

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors are in parentheses; Lewbel (2012) standard errors are heteroskedasticity-robust; Klein & Vella (2009) standard errors are obtained via bootstrap. See Table 2 for further details.

B Estimation Algorithms

B.1 Lewbel (2012) Approach

Estimation of the empirical model

$$\ln(FDI_{it}) = X_{it}\Pi + \beta\ln(R_{it}) + \delta\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}) + \varepsilon_{it}$$

proceeds as follows:

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

B.2 Klein & Vella (2009) Approach

Estimation of the empirical model

$$\ln(FDI_{it}) = X_{it}\Pi + \beta\ln(R_{it}) + \delta\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}) + \rho_1\dfrac{S_\varepsilon(z_{it})}{S_1(z_{it})}\zeta_{1it} + \rho_2\dfrac{S_\varepsilon(z_{it})}{S_2(z_{it})}\zeta_{2it} + \widetilde{\widetilde{\varepsilon}}_{it}$$

proceeds as follows:

1. Regress $\ln(R_{it})$ on X_{it} and obtain $\widehat{\zeta}_{1it}$
2. Regress $\ln(\sum_{j\in\Omega}\omega_{ijt}R_{jt})$ on X_{it} and obtain $\widehat{\zeta}_{2it}$
3. Estimate θ_j via Poisson Pseudo Maximum Likelihood (PPML) where $\mathrm{E}\left(\widehat{\zeta}_{jit}^2\right) = \exp(z_{jit}\theta_j)$; 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 $\widehat{\widehat{\zeta}}_{jit}^2$; 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:

$$\min_{\Pi,\beta,\delta,\rho_1,\rho_2,\theta_\varepsilon} \textstyle\sum_{i,t} \left[\begin{array}{c} \ln(FDI_{it}) - X_{it}\Pi - \beta\ln(R_{it}) - \delta\ln(\sum_{j\in\Omega}\omega_{ijt}R_{jt}) \\ -\rho_1\sqrt{\exp(z_{\varepsilon it}\theta_\varepsilon)}\left(\frac{\widehat{\widehat{\zeta}}_{1it}}{\widehat{\widehat{S}}_{1it}}\right) - \rho_2\sqrt{\exp(z_{\varepsilon it}\theta_\varepsilon)}\left(\frac{\widehat{\widehat{\zeta}}_{2it}}{\widehat{\widehat{S}}_{2it}}\right) \end{array}\right]^2$$

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

$$\widehat{\varepsilon}_{it} = \ln(FDI_{it}) - X_{it}\widehat{\Pi} - \widehat{\beta}\ln(R_{it}) - \widehat{\delta}\ln(\textstyle\sum_{j\in\Omega}\omega_{ijt}R_{jt}),$$

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:

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

10. Compute standard errors via bootstrap.

Table 1. Select Review of the Pollution Haven Hypothesis Literature with Endogenous Environmental Regulation.

Study	Dependent Variable	Data	Primary Measure of Environmental Regulation	Primary Instruments
Grether et al. (2012)	Total and average pollution content of imports	10 pollutants; 48 countries and 79 ISIC 4-digit industries from 1987	Lead content of gasoline	The Human Development Index
Mulatu et al. (2010)	Industry shares	13 countries and 16 ISIC industries averaged over 1990–1994	Environmental Sustainability Index in 2001	Corruption in 1995; income in 1992; urbanization in 1997; schooling in 1990
Kellenberg (2009)	Value added of majority owned U.S. multinational affiliates	50 countries and nine industries over 1999-2003	Two survey-based responses from executives concerning environmental stringency and consistency of enforcement	Own country: arable land/agricultural worker; tractors/agricultural worker. Spatial lag of other countries in same region (weighted by GDP): land/agricultural worker; tractors/agricultural worker; public schools; capital/labor ratio; infrastructure; organized crime.
Cole and Fredriksson (2009)	Inbound FDI stocks and flows divided by aggregate GDP	13 OECD and 20 developing countries over 1982-1992	Lead content of gasoline	Total population
Levinson and Taylor (2008)	U.S. net imports divided by the value of shipments	132 3-digit manufacturing sectors from Mexico and Canada over 1977-1986	PAOC per unit of value added	The amount of a pollutant contributed by other sectors in the states in which the sector tends to locate (14 pollutants yields 14 instruments); weighted average of state per capita incomes
Cole and Elliott (2005)	U.S. outbound FDI stocks in Brazil and Mexico divided by total U.S. stocks in each country	31 (Brazil) or 36 (Mexico) 3-digit U.S. SIC industries over 1989-1994	PAOC per unit of value added	Lagged PAOC per unit of value added over 1973-1978; industry-level pollution intensity in 1987
Cole et al. (2005)	U.S. net exports as a share of value added	3-digit U.S. SIC industries over 1978-1992, except 1979 and 1987	PAOC per unit of value added	Follow Levinson and Taylor (2008); six types of air pollution yields six instruments
Jug and Mirza (2005)	Imports as a share of domestic sales	Nine 2-digit ISIC industries; 12 importing countries from the EU15 and 19 exporting countries from the EU15 and Central and Eastern Europe over 1996-1999	Environmental expenditures for total manufacturing	Total public expenditure; lagged investment in environmental equipment; lagged wages
Ederington et al. (2004)	U.S. imports divided by the value of shipments	394 4-digit U.S. SIC industries fover 1978-1994 except 1979 and 1987	PAOC per unit of total materials costs	Similar to Levinson and Taylor (2008) based on geographic dispersion of industries
List et al. (2004)	Number of manufacturing plant modifications and closures	New York State county-level data over 1980-1990	Ozone attainment status	Proportion of all contiguous western neighbors that are out of attainment
List et al. (2003)	Number of new manufacturing plants	New York State county-level data over 1980-1990	Ozone attainment status	Proportion of all contiguous western neighbors that are out of attainment
Fredriksson et al. (2003)	U.S. state-level inbound FDI stocks across states	U.S. state-level panel data from four manufacturing sectors over 1977-1986	Levinson (2001) index of state-level relative PAOC	Per capita GSP and the share of legal services in GSP; non-military government employment and the interaction between non-military government employment and share of legal services in GSP; corruption and its interaction with tax effort; corruption squared and its interaction with tax effort

Table 1 (cont.). Select Review of the Pollution Haven Hypothesis Literature with Endogenous Environmental Regulation.

Study	Dependent Variable	Data	Primary Measure of Environmental Regulation	Primary Instruments
Ederington and Minier (2003)	U.S. net imports divided by the value of shipments	374 4-digit U.S. SIC industries over 1978-1992, except 1979 and 1987	PAOC per unit of total materials costs	Four-firm concentration ratio; number of firms; value of shipments; percentage of unionized workers; industry unemployment rates; lagged changes in import and export penetrations; recent industry growth; lagged total trade
Cole and Elliott (2003)	Net exports	Four manufacturing sectors in 60 countries from 1995	Index of environmental stringency from Eliste and Fredriksson (2004); proxy based on a change in energy intensity over 1980-1985 and level of energy intensity in 1980	Per capita income
Xing and Kolstad (2002)	U.S. outbound FDI	Six manfacturing sectors across 22 countries from 1985 and 1990; data for some countries for both time points, in which case the average is used, and only from one of the years for the remainder	SO_2 emissions	Infant mortality rate; population density
Henderson (1997)	Binary variable indicating whether an industry is located in a U.S. county or not	Five 3-digit U.S. SIC industries over 1978-1987 for 742 urban counties	Ozone attainment status	State fuel prices over 1978-1987; metro area manufacturing employment (except own industry) over 1978-1987; county and metro area total employment (except own industry) over 1978-1987

Table 2. Determinants of Chemical Sector FDI: OLS, Lewbel (2012), and Klein & Vella (2009) Approaches.

	Spec (1)			Spec (2)			Spec (3)			Spec (4)			Spec (5)		
	OLS	**IV**	**CF**	**OLS**	**IV**	**CF**	**OLS**	**IV**	**CF**	**OLS**	**IV**	**CF**	**OLS**	**IV**	**CF**
Panel A. Plant, Property, and Equipment															
ln(RAC)	-0.198†	-0.567*	-0.404	-0.200†	-0.359‡	-0.346	-0.153	-0.547*	-0.375	-0.222†	-0.411‡	-0.300	-0.169‡	-0.432†	-0.334
	(0.091)	(0.214)	(0.454)	(0.093)	(0.201)	(0.290)	(0.095)	(0.202)	(0.318)	(0.100)	(0.215)	(0.315)	(0.096)	(0.208)	(0.350)
ln(spatial RAC)				-0.265	-0.677	-0.032	-0.313	-1.129†	-0.673	0.323	0.313	0.321	-0.537	-1.356	1.213
				(0.178)	(0.599)	(0.775)	(0.214)	(0.469)	(0.525)	(0.286)	(0.952)	(0.905)	(0.530)	(1.005)	(1.547)
Underid Test		0.000			0.002			0.000			0.007			0.000	
F-stat		16.759			6.134			12.327			4.317			15.452	
Overid Test		0.842			0.002			0.713			0.103			0.574	
Endogeneity		0.032			0.749			0.032			0.501			0.225	
Joint Sign. Endog.	0.031	0.069	0.374	0.020	0.000	0.491	0.129	0.052	0.291	0.006	0.084	0.568	0.112	0.209	0.537
N	563	563	563	563	563	563	563	563	563	563	563	563	563	563	563
Panel B. Employment															
ln(RAC)	-0.397*	-0.836*	-0.468	-0.386*	-0.678*	-0.562	-0.291*	-0.673*	-0.619‡	-0.379*	-0.910*	-0.558	-0.345*	-0.695*	-0.267
	(0.074)	(0.161)	(0.572)	(0.070)	(0.152)	(0.345)	(0.066)	(0.141)	(0.359)	(0.075)	(0.200)	(0.432)	(0.071)	(0.162)	(0.316)
ln(spatial RAC)				-0.344†	-0.868†	0.002	-0.273‡	-0.809*	-1.380‡	0.270	-0.481	-0.582	-0.311	-1.133	0.452
				(0.143)	(0.376)	(0.849)	(0.160)	(0.297)	(0.749)	(0.250)	(0.865)	(0.949)	(0.421)	(0.853)	(1.095)
Underid Test		0.000			0.000			0.000			0.000			0.000	
F-stat		45.390			15.628			20.698			8.045			17.304	
Overid Test		0.186			0.031			0.223			0.743			0.398	
Endogeneity		0.000			0.029			0.004			0.001			0.024	
Joint Sign. Endog.	0.000	0.000	0.413	0.000	0.000	0.243	0.000	0.000	0.093	0.000	0.000	0.405	0.000	0.001	0.666
N	621	621	621	621	621	621	621	621	621	621	621	621	621	621	621

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors are in parentheses; OLS and IV standard errors are heteroskedasticity-robust and CF standard errors are obtained via bootstrap. 'IV' refers to the Lewbel (2012) approach estimated via GMM; 'CF' refers to the Klein and Vella (2009) control function approach. Other covariates included in Specification 1 include: average production-worker wages, land prices, energy prices, total road mileage, unemployment rate, unionization rate, market proximity, population, tax effort, state dummies, and year dummies. Specifications 2 - 5 also include spatial versions of these controls. Excluded instruments in the IV estimations are land values, market proximity, and road mileage demeaned and interacted with the first-stage residuals (Specification 1) and the spatial counterparts (Specifications 2 - 5). Underid reports the p-value of the Kleibergen-Paap (2006) rk statistic with rejection implying identification; Overid reports the p-value of Hansen J statistic with rejection casting doubt on instruments' validity; Endog reports the p-value of endogeneity test of the endogenous regressors; Joint Sign. reports the p-value of Anderson-Rubin (1949) chi-square test of endogenous regressors; F-stat reports the Kleibergen-Paap F statistic for weak identification. See text for further details.

Table 3. Determinants of Total Manufacturing FDI: OLS, Lewbel (2012), and Klein & Vella (2009) Approaches.

	Spec (1)			Spec (2)			Spec (3)			Spec (4)			Spec (5)		
	OLS	**IV**	**CF**	**OLS**	**IV**	**CF**	**OLS**	**IV**	**CF**	**OLS**	**IV**	**CF**	**OLS**	**IV**	**CF**
Panel A. Plant, Property, and Equipment															
ln(RAC)	-0.079	-0.143	-0.085	-0.082	-0.234†	-0.140	-0.011	0.043	-0.078	-0.086‡	-0.094	-0.101	-0.075	-0.110	-0.063
	(0.054)	(0.098)	(0.212)	(0.054)	(0.099)	(0.162)	(0.050)	(0.088)	(0.145)	(0.052)	(0.096)	(0.137)	(0.051)	(0.089)	(0.137)
ln(spatial RAC)				-0.127	-1.119*	0.568	-0.094	-0.239	0.321	0.181	0.626	-0.334	0.117	-0.308	0.863
				(0.096)	(0.278)	(0.487)	(0.100)	(0.190)	(0.290)	(0.161)	(0.447)	(0.622)	(0.281)	(0.528)	(0.986)
Underid Test		0.000			0.000			0.000			0.000			0.000	
F-stat		86.485			12.790			30.768			10.615			16.062	
Overid Test		0.799			0.446			0.045			0.758			0.056	
Endogeneity		0.318			0.000			0.632			0.395			0.726	
Joint Sign. Endog.	0.143	0.484	0.689	0.112	0.000	0.370	0.635	0.065	0.405	0.103	0.409	0.672	0.320	0.092	0.647
N	811	811	811	811	811	811	811	811	811	811	811	811	811	811	811
Panel B. Employment															
ln(RAC)	-0.013	0.036	0.250	-0.027	-0.031	0.108	0.053	0.168†	0.110	-0.042	-0.048	0.084	-0.013	0.101	0.128
	(0.057)	(0.119)	(0.260)	(0.054)	(0.095)	(0.182)	(0.054)	(0.083)	(0.129)	(0.057)	(0.125)	(0.162)	(0.052)	(0.102)	(0.169)
ln(spatial RAC)				0.040	-0.173	0.287	-0.023	0.016	0.243	0.201	-0.047	-0.478	0.170	0.369	1.062‡
				(0.100)	(0.234)	(0.276)	(0.107)	(0.161)	(0.218)	(0.136)	(0.389)	(0.301)	(0.253)	(0.475)	(0.568)
Underid Test		0.000			0.000			0.000			0.000			0.000	
F-stat		99.271			14.646			32.756			10.882			15.934	
Overid Test		0.681			0.250			0.389			0.949			0.923	
Endogeneity		0.538			0.503			0.132			0.772			0.318	
Joint Sign. Endog.	0.820	0.845	0.336	0.740	0.381	0.519	0.488	0.206	0.434	0.244	0.991	0.166	0.773	0.912	0.161
N	814	814	814	814	814	814	814	814	814	814	814	814	814	814	814

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Standard errors are in parentheses; OLS and IV standard errors are heteroskedasticity-robust and CF standard errors are obtained via bootstrap. See Table 2 for further details.

Table 4. Tests for Heteroskedasticity in First-Stage Equations in Klein and Vella (2009) Approach.

	Chemical Sector					Total Manufacturing				
	Spec (1)	**Spec (2)**	**Spec (3)**	**Spec (4)**	**Spec (5)**	**Spec (1)**	**Spec (2)**	**Spec (3)**	**Spec (4)**	**Spec (5)**
Panel A. Plant, Property, and Equipment										
Before: ln(RAC)										
Test Statistic	37.000	52.355	60.358	53.723	64.335	86.393	110.925	115.225	109.317	118.178
P-Value	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000
After: ln(RAC)										
Test Statistic	4.060	8.775	11.896	10.189	11.607	11.220	19.097	13.861	14.533	13.984
P-Value	p = 0.669	p = 0.722	p = 0.454	p = 0.599	p = 0.478	p = 0.082	p = 0.086	p = 0.310	p = 0.268	p = 0.302
Before: ln(spatial RAC)										
Test Statistic		30.922	84.142	43.975	34.732		62.267	111.974	60.608	81.779
P-Value		p = 0.002	p = 0.000	p = 0.000	p = 0.001		p = 0.000	p = 0.000	p = 0.000	p = 0.000
After: ln(spatial RAC)										
Test Statistic		8.523	29.396	12.540	23.200		6.248	24.528	11.182	38.601
P-Value		p = 0.743	p = 0.003	p = 0.403	p = 0.026		p = 0.903	p = 0.017	p = 0.513	p = 0.000
Panel B. Employment										
Before: ln(RAC)										
Test Statistic	63.212	69.095	75.143	85.399	64.877	92.999	114.835	120.662	118.754	123.462
P-Value	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000	p = 0.000
After: ln(RAC)										
Test Statistic	5.921	14.170	9.667	10.641	21.451	11.798	17.338	12.771	13.383	12.461
P-Value	p = 0.432	p = 0.290	p = 0.645	p = 0.560	p = 0.044	p = 0.067	p = 0.137	p = 0.386	p = 0.342	p = 0.409
Before: ln(spatial RAC)										
Test Statistic		46.322	96.365	48.607	58.834		63.573	115.958	62.431	79.037
P-Value		p = 0.000	p = 0.000	p = 0.000	p = 0.000		p = 0.000	p = 0.000	p = 0.000	p = 0.000
After: ln(spatial RAC)										
Test Statistic		9.840	21.643	12.653	32.317		6.042	24.875	11.615	37.940
P-Value		p = 0.630	p = 0.042	p = 0.395	p = 0.001		p = 0.914	p = 0.015	p = 0.477	p = 0.000

Notes: Test statistics and corresponding p-values are from Koenker's (1981) test for heteroskedasticity. 'Before' corresponds to the test of residuals from the initial OLS estimation of the first-stage equations (Steps 1 and 2 in Appendix B.2). 'After' corresponds to the test of the residuals after the FGLS estimation (Step 4 in Appendix B.2). Test statistic is distributed as χ^2 with 6 degrees of freedom in Specification 1 and 12 degrees of freedom in Specifications 2 - 5. See text for further details.

Table 5. JIVE Estimation of the Lewbel (2012) Approach.

	Chemical Sector					Total Manufacturing				
	Spec (1)	**Spec (2)**	**Spec (3)**	**Spec (4)**	**Spec (5)**	**Spec (1)**	**Spec (2)**	**Spec (3)**	**Spec (4)**	**Spec (5)**
Panel A. Plant, Property, and Equipment										
ln(RAC)	-1.539‡	-0.791	-4.432	-1.243	-3.603	-0.176	3.341	-0.020	-0.627	-0.174
	(0.906)	(0.582)	(13.223)	(1.329)	(9.616)	(0.135)	(11.265)	(0.128)	(1.152)	(0.130)
ln(spatial RAC)		0.506	-7.427	0.978	-22.515		17.880	-0.226	-4.745	-0.330
		(0.586)	(22.270)	(1.120)	(89.193)		(57.838)	(0.358)	(12.348)	(0.624)
Joint Sign. Endog.	0.090	0.175	0.738	0.350	0.708	0.193	0.767	0.877	0.586	0.181
N	563	563	563	563	563	811	811	811	811	811
Panel B. Employment										
ln(RAC)	-1.092*	-0.913*	-1.009*	-0.260	-1.183*	0.067	-0.201	0.224	0.560	0.142
	(0.276)	(0.246)	(0.216)	(0.536)	(0.415)	(0.176)	(1.284)	(0.149)	(1.620)	(0.189)
ln(spatial RAC)		-0.658	-1.677*	6.218†	-4.292		-1.610	0.098	6.129	0.797
		(1.183)	(0.471)	(2.687)	(2.899)		(7.405)	(0.302)	(18.379)	(1.383)
Joint Sign. Endog.	0.000	0.000	0.000	0.628	0.005	0.701	0.875	0.132	0.730	0.451
N	621	621	621	621	621	814	814	814	814	814

Notes: ‡ p<0.10, † p<0.05, * p<0.01. Heteroskedasticity-robust standard errors are in parentheses. See Table 2 for further details.

Table 6. Determinants of Chemical Sector FDI: Alternative Instruments in the Lewbel (2012) Approach.

	Specification (1)						Specification (5)					
	Baseline	**IV Set A**	**IV Set B**	**IV Set C**	**IV Set D**	**IV Set E**	**Baseline**	**IV Set A**	**IV Set B**	**IV Set C**	**IV Set D**	**IV Set E**
Panel A. Plant, Property, and Equipment												
ln(RAC)	-0.567*	-0.368	-0.641†	-0.637†	-0.486‡	-0.454†	-0.432†	2.384	-0.947	-0.548	-0.247	-0.341†
	(0.214)	(0.704)	(0.295)	(0.300)	(0.258)	(0.182)	(0.208)	(62.550)	(1.083)	(0.352)	(0.232)	(0.157)
ln(spatial RAC)							-1.356	29.205	11.223	0.093	0.744	0.559
							(1.005)	(72.667)	(24.446)	(1.763)	(1.439)	(0.635)
Underid Test	0.000	0.358	0.000	0.000	0.000	0.000	0.000	0.969	0.607	0.050	0.075	0.000
F-stat	16.759	0.709	31.711	26.152	10.440	13.088	15.452	0.001	0.113	2.570	2.071	10.367
Overid Test	0.842				0.116	0.460	0.574				0.020	0.062
Endogeneity	0.032	0.810	0.065	0.084	0.244	0.097	0.225	0.379	0.283	0.214	0.654	0.084
Joint Sign. Endog.	0.069	0.651	0.024	0.032	0.037	0.113	0.209	0.471	0.186	0.161	0.024	0.031
N	563	563	563	563	563	563	563	563	563	563	563	563
Panel B. Employment												
ln(RAC)	-0.836*	-0.973*	-0.908*	-0.784*	-0.580*	-0.546*	-0.695*	-1.112†	-0.833*	-0.632*	-0.307‡	-0.343*
	(0.161)	(0.302)	(0.171)	(0.180)	(0.153)	(0.138)	(0.162)	(0.567)	(0.287)	(0.201)	(0.157)	(0.127)
ln(spatial RAC)							-1.133	-6.031	-1.373	0.370	1.300	0.414
							(0.853)	(6.468)	(4.266)	(1.328)	(1.181)	(0.635)
Underid Test	0.000	0.004	0.000	0.000	0.000	0.000	0.000	0.315	0.238	0.003	0.047	0.000
F-stat	45.390	7.325	112.814	63.549	24.799	36.317	17.304	0.461	0.598	6.778	2.681	14.753
Overid Test	0.186				0.002	0.001	0.398				0.005	0.007
Endogeneity	0.000	0.004	0.000	0.004	0.091	0.093	0.024	0.070	0.005	0.023	0.335	0.421
Joint Sign. Endog.	0.000	0.001	0.000	0.000	0.000	0.000	0.001	0.024	0.000	0.001	0.001	0.000
N	621	621	621	621	621	621	621	621	621	621	621	621

Notes: ‡ $p<0.10$, † $p<0.05$, * $p<0.01$. Heteroskedasticity-robust standard errors are in parentheses. Baseline model is repreated from Table 2. IV Set A uses own (spatially lagged) land values in z_1 (z_2); IV Set B uses own (spatially lagged) market proximity in z_1 (z_2); IV Set C uses own (spatially lagged) road mileage in z_1 (z_2); IV Set D uses own (spatially lagged) population, unemployment rate, and unionization rate in z_1 (z_2); and, IV Set E uses own (spatially lagged) land values, market proximity, road mileage, population, unemployment rate, and unionization rate in z_1 (z_2); See Table 2 for further details.

Table 7. Determinants of Relative FDI: Pitt & Rosenzweig (1990) Approach.

	Spec (1)		Spec (2)		Spec (3)		Spec (4)		Spec (5)	
	OLS	**IV**	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**	**OLS**	**IV**
Panel A. Plant, Property, and Equipment										
ln(RAC)	-0.789*	-4.452*	-0.726*	-4.507*	-0.802*	-3.273*	-0.657*	-3.198*	-0.592*	-3.142*
	(0.155)	(0.571)	(0.146)	(0.603)	(0.142)	(0.430)	(0.151)	(0.481)	(0.137)	(0.533)
ln(spatial RAC)			-1.052*	0.676	0.596	0.840	0.148	4.759†	-1.843	-2.562
			(0.350)	(1.346)	(0.498)	(1.508)	(0.576)	(2.355)	(1.859)	(4.509)
Underid Test		0.000		0.000		0.000		0.000		0.001
F-stat		39.174		8.684		6.344		9.340		3.284
Overid Test		0.029		0.111		0.053		0.010		0.014
Endogeneity		0.000		0.000		0.000		0.000		0.000
Joint Sign. Endog.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N	563	563	563	563	563	563	563	563	563	563
Panel B. Employment										
ln(RAC)	-0.498*	-2.159*	-0.447*	-2.171*	-0.494*	-1.433*	-0.584*	-2.106*	-0.456*	-1.655*
	(0.096)	(0.295)	(0.096)	(0.298)	(0.099)	(0.280)	(0.101)	(0.304)	(0.090)	(0.271)
ln(spatial RAC)			-0.759*	0.323	0.096	1.857†	-0.546	1.577	-2.102†	-1.548
			(0.211)	(0.702)	(0.277)	(0.830)	(0.398)	(1.613)	(1.011)	(2.424)
Underid Test		0.000		0.000		0.000		0.000		0.000
F-stat		38.003		13.321		10.666		10.037		5.481
Overid Test		0.834		0.166		0.156		0.134		0.242
Endogeneity		0.000		0.000		0.000		0.000		0.000
Joint Sign. Endog.	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
N	621	621	621	621	621	621	621	621	621	621

Notes: ‡ $p<0.10$, † $p<0.05$, * $p<0.01$. Standard errors are heteroskedasticity-robust. IV estimation is via GMM. Dependent variable is chemical sector FDI minus all other manufacturing FDI. RAC is relative abatement costs. Other covariates included in Specification 1 include: average production-worker wages, land prices, energy prices, total road mileage, unemployment rate, unionization rate, region dummies, and year dummies. Specifications 2 - 5 also include spatial versions of these controls. Specification 2 uses contiguous weights; Specification 3 uses weights based on U.S. Census regions; Specification 4 uses weights based on Crone's (1998/1999) regions; Specification 5 uses distance-based weights. Excluded instruments are market proximity, population, and tax effort (Specifications 1 - 5) and the spatial counterparts (Specifications 2 - 5). Underid reports the p-value of the Kleibergen-Paap (2006) rk statistic with rejection implying identification; Overid reports the p-value of Hansen J statistic with rejection casting doubt on instruments' validity; Endog reports the p-value of endogeneity test of the endogenous regressors; Joint Sign. reports the p-value of Anderson-Rubin (1949) chi-square test of significance of the endogenous regressors; F-stat reports the Kleibergen-Paap F statistic for weak identification. See text for further details.