Examining the Evidence on Environmental Regulations and Industry Location

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This article offers a review and critique of the large literature on the pollution havens hypothesis. This hypothesis refers to the notion that certain jurisdictions can become pollution havens as dirty industries relocate or expand in response to differences in regulatory stringency. The early literature, based on cross-sectional analyses, typically concludes that environmental regulations have an insignificant effect on firm location decisions. However, recent studies that use panel data to control for unobserved heterogeneity, or instruments to control for endogeneity, find statistically significant pollution haven effects of reasonable magnitude. Furthermore, this distinction appears regardless of whether the studies look across countries, states, counties, or industries, or whether they examine plant locations, investment, or international trade patterns.

Keywords: *pollution haven; industrial flight; trade and environment*

Although the trade versus environment debate is not new, it has taken a particularly heated form in recent years. The debate's roots can be traced back to the early 1970s when environmental laws in industrialized countries sparked concerns about how differences in national pollution control standards might influence trade patterns and industry location decisions. Public and research interest dimmed somewhat in the 1980s, only to be renewed in the early 1990s by the United Nations Conference on Environment and Development (UNCED), the Uruguay round of the Generalized Agreements on Tariffs and Trade (GATT), and the North American Free Trade Agreement (NAFTA) negotiations. The debate made front-page news by the end of the decade, with violent demonstrations at World Trade Organization (WTO) meetings in Seattle and Genoa. These protests were ignited, in part, by concerns about the environmental effects of increased world trade. To address some of these concerns, U.S. Presidential Executive Order 13141, signed in 1999,

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requires U.S. agencies to conduct quantitative assessments of the environmental effects of pending trade liberalization agreements.

The wide variety of constituencies involved in the trade versus environment debate have an equally broad array of concerns. Some environmentalists fear that liberalized trade will lead to an increase in pollution—especially in developing countries—directly through an increase in the scale of economic activity and transportation and indirectly though changes in the composition of industries.[1] Furthermore, if all countries strive to attract capital, there may be a race to the bottom of environmental standards. Some free trade advocates fear, in turn, that environmental concerns serve as a guise for protectionism and can be used as nontrade barriers. Domestic manufacturing interests in developed countries also worry that strict environmental regulations will drive manufacturing overseas. All of these concerns—about foreign environmental quality, about tariffs and environmental regulations being substitutes, and about the fate of manufacturers facing strict regulations—hinge on the assumption that industry is sensitive to interjurisdictional differences in regulatory stringency. This assumption has been called the *pollution haven hypothesis* and essentially refers to the trade-induced composition effect previously described.

A number of researchers have attempted to measure this pollution haven effect. Detailed surveys of early empirical studies of industry location decisions can be found in Dean (1992), Jaffe, Peterson, Portney, and Stavins (1995), and Levinson (1996a). Each survey concludes that, contrary to common perception, environmental regulations have little effect on location decisions. The most commonly offered explanation is that compliance costs are too small—relative to other costs—to have a significant effect on industry location decisions.

These findings have done little to placate public concern. A 1999 opinion survey found 67% of respondents felt that the absence of international environmental standards threatens U.S. jobs as well as the environment in developing countries because lower environmental standards abroad induce U.S. companies to relocate (Coughlin, 2002). This concern, coupled with growing domestic compliance costs, has motivated a number of researchers to re-examine the methodological approaches of the early studies.[2]

The earlier consensus that regulatory differences do not matter is beginning to change for several reasons. First, only recently have papers

1. An economy of the same size can produce different levels of pollution depending on the relative size of its various agricultural, manufacturing, and service sectors. If dirtier industries migrate from developed to developing countries, the composition of both economies will change. Even if there is no relocation, domestic manufacturing firms in developing countries can expand in response to world demand.

2. Pollution abatement operating costs grew by about 48% between 1982 and 1990 in the United States (Brunnermeier & Cohen, 2003).

used panels of data on regulatory stringency, which allow researchers to control for unobserved attributes of countries or industries that are correlated with regulatory stringency and economic strength. Second, only the most recent articles have explicitly recognized that regulatory stringency may be endogenous and have attempted to instrument for stringency to separate the effect of stringency on trade without the confounding effect of trade on stringency. In this review, we summarize important contributions to this literature, including recent papers that do find a statistically significant effect of environmental regulations on trade and investment, and attempt to interpret and explain the new results.

The Typical Empirical Strategy

The theoretical starting point for most of the empirical investigations in this area is the Heckscher-Ohlin model of international trade, which shows that countries tend to export goods whose production is intensive in locally abundant factors of production. Because this theory does not yield estimable structural relationships between pollution regulations and trade, empirical articles have typically relied on reduced-form regressions of trade flows on factor endowments and other country characteristics. An example would be,

$$Y_i = \alpha F_i + \beta R_i + \gamma T_i + \varepsilon_i \tag{1}$$

where the dependent variable Y is some measure of economic activity (such as net exports, new plant openings, employment, or foreign direct investment [FDI]) in a country, F is a vector of factor endowments, R is the stringency of environmental regulations, T is trade barriers, and ε is a random error term.

The basic claim of the Heckscher-Ohlin model (countries export products using abundantly endowed factors) suggests several ways of identifying an equation such as Equation 1. First, one could examine trade among countries, where Y_i represents, say, net exports from country i, and the regressors are country characteristics. Alternatively, one could examine differences across industries for a single country. In this case Y_i would represent net exports for industry i, and the regressors would be industry characteristics. Or, one could examine investment or employment, rather than trade. Presumably if a country or industry is to export more of a product, it will require more investment and more employees. In this case, Y_i would be any measure of investment, number of new plant births, number of employees, and so forth.

Equation 1 can also be used to measure the effect of environmental regulatory differences on economic activity at the level of states rather than nations, or at the substate level, such as counties. Thus, *Y* could just as easily be interpreted as new investment in state or county *i*, whereas *F* could represent endowments (such as skilled labor and infrastructure), and *R* could be regulatory stringency in that jurisdiction. *T* would likely drop out of the model if there are no explicit barriers to interjurisdictional commerce.

One could also imagine trying to combine the cross-country interpretation of Equation 1 with the cross-industry interpretation. In other words, with sufficient data we could regress trade flows by industry and country on industry and country characteristics and their interactions. In this case, we would expect the countries with the least strict environmental regulations to have the largest net exports from the most pollution intensive industries. In other words, the coefficient on an interactive term between country regulatory stringency and industry pollution intensity would be negative. Perhaps because of the data requirements of such an exercise, only Smarzynska and Wei (2001) attempted to measure this interaction.

Examples of variables that could be used in this basic model are summarized in Table 1. Several caveats must be kept in mind when reading this table. First, economic theory does not specify the functional form of the relationships. This affects the confidence that readers can place on the findings of studies that fail to test for the robustness of their results to alternative specifications. Second, the existing empirical literature yields no consensus estimates on the direction and magnitude of the coefficients of the various right-hand side variables. Finally, there is no reason to assume—even in theory—that the direction of the relationship runs only from the explanatory variables (factor endowments, regulatory stringency, and trade barriers) to the dependent variable (economic activity). We argue later that this might explain some of the counterintuitive results reported in the literature.

DEFINITIONS

Although empirical studies estimating Equation 1 describe themselves as tests of the pollution haven hypothesis, the studies vary in how they implicitly frame that hypothesis. They also differ from how that hypothesis has been framed by the public debate. Consequently, we would like to propose the following three definitions of a pollution haven effect.

Definition 1: Economic Activity Shifts to Jurisdictions With Less Strict Environmental Regulations.

Table 1
Model Specification Options

Variable	*Manifestation*
Economic activity (*Y*)	Net exports
	Foreign direct investment
	Plant closures or births
	Employment
	Output
Factor endowments (*F*)	Human capital
	Physical capital
	Land
	Minerals
	Energy costs
	Infrastructure

This claim is the most straightforward: Controlling for differences in factor endowments, jurisdictions that have weaker environmental regulations will exhibit more economic activity. Looking back at Equation 1, this effect would be captured by $\partial Y/\partial R<0$. Most of the empirical literature on the pollution haven effect tests this hypothesis. Note that this hypothesis makes no normative claim about the efficiency of outcomes, only about the sensitivity of investment and trade to regulatory differences.

Definition 2: Trade Liberalization Encourages an Inefficient Race to the Bottom.

In theory, a government that wants to set an efficient environmental regulation should choose the level of stringency such that the benefit of the regulation justifies its cost at the margin. However, different countries can place different values on a unit reduction in emissions of the same pollutant, without necessarily leading to an economically inefficient level of pollution. Assimilative capacity (the local environment's ability to tolerate pollution) differs across countries. The cost of a given level of pollution abatement can also differ across countries because of differences in factor prices, technology, or geography. Thus, as argued in Bhagwati (1993), there should be little cause for concern (from an efficiency perspective) if individual countries set differing environmental standards for local pollutants, as long as these standards are locally optimal and there are no global or transboundary externalities.

There would be cause for concern, however, if trade liberalization causes individual countries competing for mobile capital to set purposely suboptimal standards. A number of theoretical articles have modeled situations in which interjurisdictional competition for investment can lead to suboptimal environmental standards (see, for example, Levinson, 1997; Markusen, Morey, & Olewiler, 1995; or Oates & Schwab, 1988).

This definition has the clearest normative implications. However, no researcher to date has attempted to test this definition because one would need to know the efficient level of regulation in different regions to judge whether observed locally set regulations are suboptimal. Several recent articles test for and find strategic behavior in the United States (Levinson, 2003; Fredriksson & Millimet, 2002). However, these articles are silent as to whether that behavior leads to overly stringent, overly lax, or efficient standards.

Definition 3: Trade Liberalization Shifts Polluting Economic Activity Toward Countries That Have Less Strict Environmental Standards.

Note the subtle difference between this and the first definition. Definition 1 claims that environmental regulations affect trade. Definition 3 claims that trade barriers affect trade in pollution-intensive goods, and hence the environment. It seems that this would only be true if the trade barriers differentially affect polluting and clean industries.

To test this, we can rewrite Equation (1) to include an interaction term:

$$Y_i = \alpha F_i + \beta R_i + \gamma T_i + \theta R_i T_i + \varepsilon_i \quad (2)$$

This indirect effect of trade barriers on pollution would be represented by $\theta = \partial\, \partial Y/[\partial R\, \partial T]$, where R is environmental regulatory stringency and T measures barriers to trade. Recall that Definition 1 describes the direct effect of stricter environmental regulations on economic activity. In contrast, Definition 3 captures the indirect effect of a trade liberalization agreement on the direct effect described in Definition 1. Definition 3 is also more descriptive than it is normative; that is, although it searches for a trade and environmental policy induced redistribution of economic activity between regions, it is silent about the welfare consequences of this redistribution.

Given the complexity of measuring the cross effect between trade liberalization and regulatory stringency, few articles have attempted to test this third hypothesis. Only Lucas, Wheeler, and Hettige (1992), and Birdsall and Wheeler (1993) have made preliminary attempts to test the effect of trade liberalization on the level of economic activity in polluting industries.

COMPLICATIONS IN ESTIMATION

Two issues complicate empirical tests of Equation 1: unobserved heterogeneity and endogeneity. The first of these is that some unobserved industry or country characteristics are likely to be correlated with the propensity to impose strict regulations and the propensity to manufacture and export polluting goods. The omission of these unobserved variables in a simple cross-section model will lead to inconsistent results that cannot be meaningfully interpreted. For example, if some country has an unobserved comparative advantage in the production of a polluting good, it will export a lot of that good, generate a lot of pollution, and (all else equal) impose strict regulations to control the pollution. A cross-section comparison will find strict pollution regulations positively correlated with exports and may be mistakenly interpreted as support for the Porter hypothesis that stricter environmental regulations promote competitiveness (Porter & van der Linde, 1995).

The simplest way to account for this unobserved heterogeneity would be to collect panel data and incorporate country/state/county/industry fixed effects (ν_i):

$$Y_{it} = \nu_i + \alpha F_{it} + \beta R_{it} + \gamma T_{it} + \varepsilon_{it} \quad (3)$$

The fixed effects capture unobservable heterogeneity or inherent characteristics that vary across observations but are constant over time. Examples of country fixed effects include sources of comparative advantage, natural resources, proximity to markets, natural harbors, and investment friendly business climates. Examples of industry fixed effects include low transportation costs and geographic "footlooseness."[3]

The second issue complicating these analyses is that pollution regulations and trade may be endogenous, that is, there are causal relationships running in both directions. For example, if greater economic activity leads to higher income, and higher income leads to greater demand for environmental quality, then environmental regulations could be a function of trade. The typical solution to this source of bias is to employ an instrumental variable approach to account for the endogeneity of pollution abatement policy, as described in Ederington and Minier (2003), Levinson and Taylor (2003), and Xing and Kolstad (2002). The model can be solved using two-stage least squares. The intuition is to find instruments that vary over time and are correlated with the measure of regulatory stringency (*R*), conditioned on the other exogenous variables, but not with the error term ε_{it} in Equation 3.

3. Geographic footlooseness refers to the notion that some industries may have high factor or product transportation costs and may be strongly tied to particular locations (Ederington et al., 2003).

With respect to Definition 3, this issue of endogeneity is complicated further by the fact that in theory, tariffs and pollution taxes can be sometimes used as substitute policy instruments. Although a tariff is not the first-best policy for achieving local environmental objectives because it does not directly attack the source of the local distortion, it could be used as a second-best policy instrument (Bhagwati, 1987). Similarly, Copeland and Taylor (in press) pointed out that environmental policy is sometimes used as an (imperfect) substitute for trade policy to protect local firms, especially when tariffs and quotas are constrained by trade agreements. To the extent that these instruments are used as substitutes by real-world policy makers, researchers will also need to account for $\partial R/\partial T$ or $\partial T/\partial R$ when using Definition 3 to measure pollution haven effects. Only Ederington and Minier (2003) attempt to address this issue directly.

Prior Literature

In the past, researchers have adopted a variety of strategies to test for pollution havens. The simplest investigations attempt to gather primary data on factors governing location decisions by interviewing industry representatives. Examples of these studies are presented in the section on industry interviews. More recently, researchers have moved beyond surveys and have conducted econometric analyses to account for the differences between what firms say and what they actually do. These econometric studies can be classified into three broad categories: direct examinations of location choice and indirect examinations of output and input flow. The first category of studies has largely focused on interjurisdictional competition for the siting of new plants within the United States due to a lack of comparable cross-country data. This branch of the literature is summarized in the section on effect on location choice. The second class of empirical studies focuses on changes in output (such as final goods or emissions). It tests whether environmental regulations affect patterns of specialization and trade. These studies are described in the section on effect on output. The third approach, summarized in the section on effect on inputs, tests whether environmental policy affects the movement of inputs (such as capital or labor) across regions instead of across industries.

INDUSTRY INTERVIEWS

A number of surveys have been conducted during the last 2 decades to try to understand industry location decisions. Some are summarized in Table 2. For example, a U.S. General Accounting Office survey in 1991

Table 2
Industry Interviews on Location Decisions

Study	*Sample*	*Findings*
Fortune (1977)	1,000 largest U.S. corporations, 1977	11% ranked state or environmental regulations among top 5 criterion for choosing between U.S. sites.
Stafford (1985)	162 branch plants built in late 1970s and early 1980s in United States	Environmental regulations were not a major factor but were more important than in the 1970s. When only self-described "less clean" plants were examined, environmental factors were of "mid-level importance."
Lyne (1990)	*Site Selection* magazine's survey of U.S. corporate real estate executives, 1990	When asked to select 3 of 12 listed factors affecting location choice, 42% of respondents picked "state clean air legislation."
U.S. General Accounting Office (1991)	2,675 wood furniture manufacturers in Los Angeles, 1988-1990	Very small proportion relocated to other areas within United States or to Mexico. Movers cited labor and environmental costs.
United Nations Conference on Trade and Development (1993)	169 corporations with sales exceeding $1 billion, 1990	Most claimed that environmental, health, and safety practices overseas were determined by home country regulations.
Abel and Phillips (2000)	3 garment finishing facilities in El Paso, 2000	Most of the 24 finishing facilities that operated in El Paso in 1993 have relocated to Mexico. Stayers believe others left due to reduced tariffs on reimportation, lower labor costs, and laxer water conservation and wastewater regulations.

found that between 11 and 28 of the 2,675 wood furniture manufacturers in Los Angeles relocated at least some part of their operations to Mexico. Although an insignificant fraction of firms migrated, the majority of those that did migrate identified labor costs and pollution control costs as significant factors affecting their decision.

Studies have also asked the related question whether firms relocate within the United States because of interstate differences in regulatory stringency. Although such surveys have consistently found that respondents did not report environmental regulation as the most important factors in their location decisions, this may be due to some form of reporting bias. Although these surveys are not directly comparable, Table 2 suggests that the relevance of environmental criteria in location decisions is inconclusive. Furthermore, it is not possible to isolate and quantify these effects based on survey responses alone. For this we must turn to empirical studies of location decisions.

EFFECT ON LOCATION CHOICE

Given the dearth of comparable international data, studies of industry location decisions have mostly explored the role of environmental factors in explaining new plant births among U.S. states or counties. Examples of these studies are summarized in Table 3. Most analyze the factors affecting the location of new plants. This is because new plants are not as constrained in their location choice by sunk costs and are arguably more sensitive to small regional differences in regulatory stringency.

Plant location decisions are typically modeled using McFadden's (1973) conditional logit framework. This model assumes that firm *i* will select location *j* if the expected profits ∂_{ij} exceed the expected profits δ_{ik} for all alternative *k* locations. The unobserved (or latent) profits for plant i at location *j* are given by:

$$\hat{\Pi}_{ij} = \delta' X_j + \mu_{ij} \qquad (4)$$

where X_j is a vector of observable state characteristics, δ is a vector of estimated parameters, and μ_{ij} is a Weibull error term.

Regional characteristics that can potentially affect location decisions include wage rates, unionization, energy costs, taxes, infrastructure, and market size. Other things equal, firms are likely to be deterred from locating in jurisdictions where the costs of inputs (such as labor and energy) are higher and attracted to those that offer corporate tax incentives. Similarly, firms should be attracted to jurisdictions with larger market size because a large market is needed for efficient utilization of resources and exploitation of economies of scale. Bartik (1988) was among the first to introduce environmental regulations as an additional, potential determinant of firm location decisions. Thus the vector X_j in Equation 2 can be thought to comprise two components: the jurisdiction's environmental regulatory attributes and other characteristics (such as wages and infrastructure) that affect location choice.

Table 3
Studies of Location Decisions

Study	*Empirical Approach*	*Dependent Variable*	*Environmental Variable*	*Control Variables*	*Findings (and critiques)*
Bartik (1988)	Cross-section, conditional logit	Location of new Fortune 500 plant branches in United States, 1972-1978.	State air pollution spending, and State water pollution spending	State characteristics including taxes, wages, unionization, education, roads, population density, energy prices, construction costs, and region dummies.	Environmental variables have insignificant effect on plant locations.
Bartik (1989)	Cross-section and panel, conditional logit	Small manufacturing business start-up rate in United States, 1976-1982.	Conservation Foundation's index	State characteristics, such as taxes, market demand, education spending, banking, and fire protection.	Environmental variable has a significant *negative* effect on start-up rates, but effect is small.
McConnell and Schwab (1990)	Cross-section, conditional logit	New vehicle assembly plants in U.S. counties, 1973-1982.	County ambient ozone attainment status	State characteristics such as taxes, production workers, wages, unionization, energy prices, and regional dummies. County characteristics including taxes and production workers.	Plants are no more likely to be located in nonattainment counties than in attainment counties. However, degree of nonattainment matters: Urban areas severely out of compliance are less likely to be chosen. Problem: small sample of 50 plants.

Friedman, Gerlowski, and Silberman (1992)	Cross-section, conditional logit	Planned foreign-owned manufacturing plant locations in United States, 1977-1988.	Aggregate statewide PAOC / gross state product from manufacturing.	State-level manufacturing wages, unionization,productivity, market access, taxes, and promotional expenses.	PAOC has insignificant effect for pooled sample, but small deterrent effect for Japanese firms. Includes no control for unobserved heterogeneity or industrial composition of states.
Henderson (1996)	Panel, fixed effects tobit and conditional Poisson	Number of plants from 5 polluting industries in U.S. counties, 1977-1987.	County ambient ozone attainment status.	County-level employment, fixed effects, and business cycle dummy.	*Fewer* plants found in nonattainment areas. Effect larger in dirtier industries.
Levinson (1996b)	Cross-section, conditional logit	New manufacturing plant locations in United States, 1982-87.	Environmental groups' indices; and/or State enforcement staff PAOC, adjusted for states' industrial composition.	State characteristics including corporate taxes, roads, energy costs, unionization, and wages	All environmental coefficients *negative*, but very few statistically significant. All small. Effect is not larger for dirtier industries.
Mani, Pargal, and Haq (1996)	Cross-section, conditional logit	New plant locations in Indian states, 1994.	Ratio of environmental spending to total spending by state government, and Number of enforcement cases at state level.	State characteristics including wages, middle school enrollment, population density, infrastructure, agglomeration, and region dummies	Environmental spending has significant positive effect on new plant siting. Effect of enforcement is insignificant. When model is rerun for only 5 polluting industries, enforcement becomes positive and significant. Counterintuitive.

(continued)

Table 3 (continued)

Study	*Empirical Approach*	*Dependent Variable*	*Environmental Variable*	*Control Variables*	*Findings (and critiques)*
Becker and Henderson (2000)	Panel, conditional Poisson	Birth of plants from 4 polluting industries in U.S. counties, 1963-1992.	County ambient ozone attainment status.	County-level real wages, manufacturing employment, fixed effects, and year dummies.	Nonattainment status has significant *deterrent* effect on plant births in these polluting industries.
List and Co (2000)	Cross-section, conditional logit	Planned new foreign-owned manufacturing plant locations in United States, 1986-1993.	State regulatory spending, and/or Aggregate statewide PAOC, List and d'Arge's index.	State attributes such as population density, number of existing plants, wages, unionization, energy cost, taxes, and promotional expenditures.	Environmental coefficients are *negative* and statistically significant. However, they are more negative and significant for cleaner industries.

Notes: FDI = foreign direct investment; PAOC = pollution abatement operating costs
Studies that find evidence of a significant negative effect of environmental regulations on location decisions are highlighted by the use of *italics* in the last column of this table.

Because we do not observe profits, all we can see is what locations the manufacturers choose. The probability that plant *i* will locate in jurisdiction *j* is given by:

$$P_{ij} = \exp(\delta' X_j) / \sum_{k=1}^{k} \exp(\delta' X_k) \tag{5}$$

The assumption that the error term is independently and identically Weibull distributed imposes the independence of irrelevant alternatives (IIA) restriction on the predicted values. This is problematic because it implies that a firm's choice between, say, New York and New Jersey is independent of what other states are options. The standard solution is to estimate a nested multinomial logit, where the nested structure of the model follows the actual decision tree made by firms. For example, if firms first decide on a region of the country, and then a state within a region, their choice between New York and Pennsylvania will be unaffected by the characteristics of California. The difficulty with this approach is that researchers must identify regressors that affect the choice of region that are different from characteristics affecting the choice of state within a region. Hence, studies of this type typically follow Bartik (1988) and take the shortcut of including regional dummies to reduce the IIA problem. The hope is that the error term is only correlated within regions and not across regions.

The early interjurisdictional studies—including Bartik (1988) and McConnell and Schwab (1990)—tend to find that other things equal, environmental variables have an insignificant effect on the siting decisions of new U.S. plants. However, all of these studies are based on cross-sections of new plant births and environmental regulations at one point in time, and the results may be biased by unobserved heterogeneity and the endogeneity of regulations.

Furthermore, other studies from this period suggest that the nature of the investment might matter. For example, Bartik (1989) finds some evidence of a significant deterrent effect on small business start-up rates. Similarly, Friedman, Gerlowski, and Silberman (1992) found that other things equal, Japanese plants are less likely to locate in states with more stringent regulations (proxied by statewide spending on abatement). List and Co (2000) found a similar deterrent effect for planned new foreign-owned manufacturing plants. Thus, foreign firms may be more geographically footloose than domestic firms. Nevertheless, these effects are all of an economically small magnitude. In other words, these studies found that the effect of environmental regulations is relatively small compared to the effect of other factors such as wages, unionization, and the size of the market. Unfortunately, these studies offer no consensus estimates on the sign and magnitude for control variables other than market size. Models that cannot provide sensible predictions for these variables ought to be viewed with suspicion.

Mani, Pargal, and Huq (1996) is the only study in this class that looks at interjurisdictional competition within a developing country—India. They estimated a cross-sectional conditional logit model using two proxies for state-level regulatory stringency: enforcement and environmental spending by state governments. They found that enforcement has an insignificant effect, but state environmental spending has a significant positive effect on new plant siting. The authors conjectured that firms view state spending as a sign of good governance rather than as a deterrent. Oddly, when the model is rerun for only the five most polluting industries, the enforcement variable becomes positive and significant. Our interpretation is that their results highlight the importance of unobserved omitted variables.

One drawback of these studies is that the use of industry-aggregated jurisdiction data can confound differences in regulatory stringency. For example, jurisdictions that attract more polluting plants (for whatever reason) will have higher abatement costs than jurisdictions with a cleaner industrial composition, even if the regulatory stringency faced by individual plants is identical across jurisdictions. Similarly, newer plants have to comply with more stringent federal regulations than existing plants. Thus, jurisdictions with relatively more new plants may report higher compliance costs than jurisdictions with older plants even if their regulations are the same. Thus, one would need to adjust reported pollution abatement costs to capture the difference in industrial composition of the jurisdiction itself. Levinson (1996b) used adjusted abatement costs as a measure of regulatory stringency when estimating conditional logit models of plant location choice. He found that industry-adjusted abatement costs have a dampening effect on plant location choices at the state level. However, the magnitude of this effect is small. Specifically, a one standard deviation increase in adjusted costs (approximately a 95% increase) reduces the probability that a plant will locate in a state with average characteristics by only 1.5%.

Another drawback of these cross-sectional studies is that the parameter estimates are based primarily on observed between-region variation in the model and ignore unobserved regional heterogeneity and changes over time. Although they are scarcer, panel data enable comparisons over time in regional attributes. The advantage is that bias caused by the omission of unobserved time-invariant variables can be mitigated by introducing regional fixed effects. Henderson (1996) recognized this limitation in the early literature and used panel data to study the effect of air quality regulations on the number of plants from five polluting industries in U.S. counties between 1978 and 1987. He noted that counties that fail to attain national ambient air quality standards (NAAQS) face more stringent requirements than counties that meet the NAAQS standard. Other things equal, this may deter firms from coming to (or remaining in) nonattainment counties. He used a dummy for the county's ground-level ozone attainment status as a measure of regulatory stringency and found that counties that meet the ozone NAAQS standard for 3 years in a row see a 7% to 9% increase in the number of plants located in the county. The effect is largest for the industrial organic chemicals industry. Henderson also re-estimated his model for five cleaner industries and found that regulatory differences have a smaller negative on location decisions in these industries than in the polluting industries. Note that this article focuses on the total stock of plants in a county and not the flow.

In contrast, Becker and Henderson (2000) examined the effect of air quality regulations on plant births in U.S. counties between 1963-1992. They estimated a conditional poisson model and found that at the county level, NAAQS nonattainment status reduced the births of new plants belonging to four heavily polluting industries by 26% to 45% during this period.

EFFECT ON OUTPUT

This section reviews the findings of the literature on the effect of regulatory differences on output measures, such as production, net exports, and emissions. These studies are summarized in Table 4. A number of researchers began studying the theoretical relationship between environmental regulations and trade patterns in the 1970s. For example, Pethig (1976) and Siebert (1977) predicted that costly abatement regulations would weaken a country's competitive position in pollution-intensive industries and diminish its net exports in these sectors. However, these theoretical predictions were not subjected to rigorous empirical tests until the late 1980s.

In one of the earliest empirical studies, Kalt (1988) used a cross-sectional Heckscher-Ohlin model to study whether domestic environmental policy affects the competitiveness of U.S. industries. The results are typical of cross-sectional studies: Pollution abatement costs have a positive effect on net exports in two-digit industries in the United States in 1977, the effect turns negative when natural resource sectors are excluded; and it becomes even more negative when the chemical industry is also excluded. These counterintuitive findings could be explained by unobserved industry heterogeneity that is not captured in a cross-sectional model.

(text continues on p. 27)

Table 4
Studies of Effect on Production, Trade, and Emissions

Study	*Empirical Approach*	*Dependent Variable*	*Environmental Variable*	*Control Variables*	*Findings (and critiques)*
Kalt (1988)	Cross-section, ordinary least squares	U.S. net exports by 2 digit sectors, 1977	U.S. PACE + PAOC, by sector	Industry characteristics such as R&D, human capital, physical capital, and unskilled labor.	Compliance costs have positive effect on net exports. Signs reversed when natural resource and chemical industries removed from sample.
Tobey (1990)	Cross-section, ordinary least squares	Net exports of 5 polluting sectors in 23 countries, 1975	UNCTAD index	Labor, land, minerals, and capital endowments at country level.	Regulatory stringency index has insignificant effect. But this index is old and subjective.
Low and Yeats (1992)	Trends	Revealed comparative advantage of country *i* in industry *j*, 1965-1988.	N/A	N/A	Dirty industries account for a growing share of exports of some developing countries. Cannot test alternative explanations for observed trade patterns.

Lucas, Wheeler, and Hettige (1992)	Pooled, ordinary least squares	Change in toxic intensity in 80 countries, 1960-1988.	N/A	Per capita income, (per capita income)2, openness index at country level.	Toxic intensity only increased for fast growing, closed economies. Problem: No controls for factor endowments. Omitted variable bias also possible because data are pooled. Also, toxic intensity index assumes similarity of technologies and enforcement across countries.
Grossman and Krueger (1993)	Cross-section, ordinary least squares	U.S. imports from Mexico, by 3-digit industry, 1987	U.S. PAOC / value added, by sector	Human capital, physical capital, tariff rate, and injury rates for U.S. sectors.	PAOC coefficients are either statistically insignificant, or negative and statistically significant—indicating more imports in those industries with lowest pollution costs.
		Value added by Maquiladora plants in 2-digit sectors, 1987	Same as above	Same as above	PAOC has negative insignificant effect.
Birdsall and Wheeler (1993)	Pooled, ordinary least squares	Change in toxic intensity in 25 Latin American countries, 1960-1988	N/A	Initial per capita income, growth in per capita income and openness index at country level.	Openness encourages cleaner industry. Issues: no controls for factor endowments. Omitted variable bias possible because data are pooled. Also, toxic intensity index assumes similarity of technologies and enforcement across countries.

(continued)

Table 4 (continued)

Study	*Empirical Approach*	*Dependent Variable*	*Environmental Variable*	*Control Variables*	*Findings (and critiques)*
van Beers and van den Bergh (1997)	Cross-section, gravity model	Bilateral trade among 21 countries for all, pollution-intensive, nonresource-based polluting sectors in 1992.	Index compiled from 1992 OECD environmental indicators, including recycling rate, energy intensity, protected areas, unleaded gas market share, and population with sewerage.	Importing and exporting country characteristics.	The environmental index is *positively* associated with imports (and this relationship is larger and more statistically significant for polluting industries). The index is negative for exports, but it is no larger for polluting industries than for the average.
Mani and Wheeeler (1998)	Graphs	Production share of dirty goods in OECD, Latin American, & Asian countries, 1960-1995.	N/A	N/A	Share of pollution intensive industries declined in OECD countries and increased in Latin America and Asia. Speculate this was the effect of changes in income, regulation and energy prices.

Osang and Nandy (2000)	Panel, dynamic model	U.S. net exports to R.O.W., by 3-digit industry, 1984-1993.	U.S. PAOC / Total investment, by sector U.S. PACE / value added, by sector.	Labor productivity and capital costs.	Operating costs *negatively* associated with U.S. exports. However, capital costs positively associated with exports. [Alternative interpretation: capital costs associated with growing exports—causality reversed].
Antweiler, Copeland, and Taylor (2001)	Panel, fixed effects	Log (SO2 concentrations) at 293 observation sites in 40 countries, 1971-1996.	N/A	Country characteristics such as GNP per capita and its square, capital-labor ratio and its square, trade intensity and interaction terms. City characteristics such as GDP/km2, temperature, and precipitation.	1% increase in country's K-L ratio leads to about a 1% point increase in pollution.
Wheeler (2001)	Graphs	Air pollutants in United States, Mexico, Brazil, and China, 1982-1998.	N/A	Inbound FDI	Most dangerous forms of air pollutants declined in a period when FDI increased. Graphs show correlation, not causation.

(continued)

Table 4 (continued)

Study	*Empirical Approach*	*Dependent Variable*	*Environmental Variable*	*Control Variables*	*Findings (and critiques)*
Dean (2002)	Panel, two-stage least squares	Water pollution discharge growth in Chinese provinces, 1987-1995.	N/A	Income growth, black market premium, relative prices, fixed effects	Technique effect outweighs composition effect. However, model has poor fit.
Ederington and Minier (2003)	Panel, three-stage least squares	U.S. net imports, by industry, 1978-1992.	U.S. PAOC / total costs, by sector Instruments for PAOC.	Industry labor and capital intensity.	PAOC has significant *positive* effect on imports. However, results are sensitive to instruments chosen, especially lagged values of net trade. Without the trade instruments, the PAOC coefficient is small.
Ederington, Levinson, and Minier (2003)	Panel, year and industry fixed effects.	U.S. net imports by 4-digit SIC code, 1978-1992.	U.S. PAOC / costs.	Industry characteristics associated with geographic mobility (footlooseness).	The most polluting industries are also the least footloose, muting the observed pollution haven effect.
Levinson and Taylor (2003)	Panel, two-stage least squares	U.S. net imports by 3-digit sector, 1977-1986.	U.S. PAOC / value added, by sector. Instruments for PAOC.	Industry fixed effects and tariff rate	Endogeneity matters. Instrumented PAOC has small *deterrent* effect on net exports.

Notes: FDI = foreign direct investment; PACE = pollution abatement capital expenditures; PAOC = pollution abatement operating costs; UNCTAD = United Nations Conference on Trade and Development; OECD = Organization for Economic Cooperation and Development; ROW = rest of the world; SIC = Standard Industrial Classification.
Studies that find evidence of a significant pollution haven effect are highlighted by the use of *italics* in the last column of this table.

Tobey (1990) used a cross-sectional Heckscher-Ohlin model to study trade patterns in five highly polluting sectors. He found that if one controls for differences in resource endowments, differences in regulatory stringency have no measurable effect on international trade patterns in these industries. However, the study consists of five cross-section regressions (one for each sector) of net exports on characteristics of 23 countries. The measure of environmental stringency is an ordinal ranking of countries, based on subjective surveys. Although it is not a significant predictor of net exports, nor are the other country characteristics.

Low and Yeats (1992) and Mani and Wheeler (1998) found that dirty industries have expanded in developing countries. However, because their analysis includes no control variables, they can only speculate about the possible explanations. In their widely referenced study on the environmental effects of NAFTA, Grossman and Krueger (1993) argued that freer trade will affect the environment by increasing the scale of economic activity, by altering the composition of economic activity, and by changing production techniques. The pollution haven hypothesis pertains to this (trade-induced) composition effect. The authors studied the relationship between economic growth and air quality and found that concentrations of sulfur dioxide and particulate matter decline after a country's per capita GDP exceeds $5,000. They dubbed it the "environmental kuznets curve" and argued that this occurs because the technique effect offsets the scale effect. In a separate exercise, the authors found that the composition effect created by further U.S.- Mexico trade was more likely to be affected by factor endowments than by differences in pollution abatement costs.

Unfortunately, only the United States has maintained a long-time series of data on emissions and pollution abatement costs at the industry level. To overcome this limitation, the World Bank has developed the industrial pollution projection system (IPPS) to infer the level of industrial pollution in foreign countries.[4] Lucas et al. (1992) used the IPPS data to examine whether the toxic intensity of production changed with economic growth for 80 countries between 1960 and 1980. They estimated a pooled cross-sectional model and found that toxic intensity of output increased in fast-growing but closed economies during this period. In contrast, fast-growing open economies shifted toward less polluting industries in the 1970s and 1980s. Birdsall and Wheeler (1993) replicated this analysis for Latin American countries and arrived at similar conclu-

4. These estimates are based on more readily obtainable indicators of industrial scale (such as the value of output, value added, or employment). Applications of IPPS data rely on the assumption that U.S. emission-to-output ratios, obtained by merging data from the Toxic Release Inventory (TRI) and other U.S. emissions data sources with the Census of Manufacturers apply to foreign countries as well.

sions. Both studies tend to focus solely on income levels and openness as the explanatory variables and ignore the role of other factors, such as resource endowments.

Although these IPPS-based studies are commendable for their broad country coverage, they assume identical, sectoral emission intensities across countries, that is, these studies assume that other determinants of sectoral pollution intensities—such as pollution control technologies, regulations, and enforcement effort—are the same across countries. This amounts to assuming away the technique effect, leaving only the scale and composition relationships between growth and environmental quality. Copeland and Taylor (in press) suggested that these studies specify the types of measurement error introduced by using the IPPS data (e.g., researchers should check if the error is correlated across time, countries, or industries).

Much of the literature described earlier assumes that environmental policy has an exogenous effect on trade. More recent work has pointed out that trade itself can endogenously affect environmental policy and industry characteristics. Bommer (1998) cited NAFTA as an example of free trade potentially improving standards abroad; an environmental side agreement (the North American Agreement on Environmental Cooperation) was used to coax labor unions into accepting the trade treaty. The Mexican Federal Attorney-General for the Environment (PROFEPA) reported that it increased its inspections of establishments under its jurisdiction from 4,600 in 1992 to 11,800 in 1997 (PROFEPA, 2001).

A correction for endogeneity significantly alters the results reported in the earlier literature. Levinson and Taylor (2003) described numerous mechanisms by which trade can alter industries' measured pollution abatement costs, including terms of trade effects, unobserved heterogeneity among industries, industry size, and natural resource intensity. When these forms of endogeneity are accounted for, the authors found that U.S. industries that experienced the largest increases in pollution abatement costs during the 1970s and 1980s also experienced the largest relative increase in net imports, thereby lending some empirical support for a trade-induced composition effect. Ederington and Minier (2003) and Ederington, Levinson, and Minier (2003) also found that pollution abatement costs have a significant positive effect on net imports when both are estimated simultaneously in a panel data model.

Antweiler, Copeland, and Taylor (2001) developed a theoretical model that divides trade's effect on the environment into scale, technique, and composition effects and test the theory using monitoring data on sulfur dioxide concentrations in 43 countries between 1971 and 1996. They extended Grossman and Krueger's (1993) work by moving beyond cross-sectional data and allowing for endogeneity. They measured the effect of trade liberalization on the composition of national output by interacting their measure of trade openness with country characteristics determining comparative advantage. A simplified version of their model is given by:

$$E_{it} = \nu i + \alpha F_{it} + \gamma T_{it} + \delta T_{it} F_{it} + \varepsilon_{it} \quad (6)$$

where E_{it} refers to environmental quality, proxied by sulfur dioxide concentrations in country i in year t, F refers to factor endowments, and T measures trade barriers. The trade intensity variable is not significant in and of itself, but when openness is interacted with country characteristics it is associated with a statistically significant but small increase in sulfur dioxide concentrations. However, when their estimates of composition, scale, and technique effects are combined, increasing trade intensity is found to be associated with an overall decline in sulfur dioxide concentrations. A similar analysis is conducted in Dean (2002) to examine the effect of trade liberalization on water pollution discharges in Chinese provinces.

EFFECT ON INPUTS

In this section, we examine the effect of regulatory differences on capital and labor. Rauscher (1997) introduced environmental externalities into a theoretical two-country model of international factor movements. His model predicts that a government can drive capital out of a country by adopting stricter environmental standards. Because domestic emission reduction is accompanied by an increase in foreign emissions, overall emissions can increase if the foreign country is less regulated.

Eskeland and Harrison (1997) found that U.S. pollution abatement costs have an insignificant effect on outbound U.S. investment as well as on inbound FDI in Côte d'Ivoire, Morocco, Venezuela, and Mexico. Smarzynska and Wei (2001) examined inbound FDI in transition economies by also using U.S. sectoral emission intensities as proxies for pollution intensities in these countries. They found that FDI is deterred from countries with stringent environmental policies, as measured by participation in international environmental treaties. Both analyses suffer from the same criticism as IPPS-based studies. In addition, the results of the latter study do not survive robustness checks using other proxies of pollution intensity or regulatory stringency.

Clark, Marchese, and Zarrilli (2000) estimated a cross-sectional logit model and found that the pollution intensity of industry output has a highly significant negative effect on the likelihood that U.S. firms conduct offshore assembly in developing countries. They argued that the United States has a comparative advantage in many dirty industries, whereas low-wage developing countries have a comparative advantage in simple labor-intensive assembly operations that are relatively clean. If labor intensity and pollution intensity are inversely correlated, the pollution haven effect may be masked. Again, this negative finding and the ex-post explanation are typical in this literature.

Berman and Bui (2001) examined the effect of direct measures of regulatory stringency on changes in employment in refineries in Los Angeles between 1979 and 1992. They found no evidence that regulations have a negative effect on employment. However, the industry they studied (refineries) is highly capital intensive. Therefore, even if a regulation has a large effect on output and investment, it might have no effect on employment in that industry.

In contrast, List and Kunce (2000) used recent (1982-1994) state-level panel data to examine the effect of environmental regulations on manufacturing employment in the U.S. chemical, paper, primary metals, and food industries. They found that environmental regulations have a modest but significant deterrent effect on job growth in the chemical, metals, and food sectors. The effect is larger in the dirtier industries than in the cleaner food industry. Similarly, Greenstone (2002) conducted a panel data analysis at the manufacturing plant level and found that relative to attainment counties, counties that were in nonattainment of federal Clean Air Act regulations lost about 590,000 jobs and $37 billion in capital stock between 1972 and 1987.

Keller and Levinson (2002) also used panel data to study foreign direct-investment inflows into the United States between 1977 and 1994. They found robust evidence that pollution abatement costs—when adjusted for state industrial composition—have a statistically significant but modest deterrent effect on the value and count of new foreign investment projects. A doubling of their industry-adjusted index of abatement cost is associated with a less than 10% decrease in foreign direct investment.

Finally, Xing and Kolstad (2002) described a measurement error problem associated with determining international differences in regulatory stringency. They used an instrumental variable approach to examine the effect of unobserved regulatory stringency on capital movement from the United States to 22 host countries in six manufacturing sectors. The (instrumented) regulatory stringency had a statistically significant deterrent effect for the two heavily polluting industries (chemicals and primary metals) and was insignificant for the less polluting industries. In contrast, a cross-section OLS model that used observed sulfur dioxide emissions as a proxy for regulatory stringency yielded biased results.

(text continues on p. 35)

Table 5
Studies of Effect on Capital and Labor

Study	*Empirical Approach*	*Dependent Variable*	*Environmental Variable*	*Control Variables*	*Findings (and critiques)*
Eskeland and Harrison (1997)	Panel, fixed effects	Inbound FDI in 4-digit sectors in Côte d'Ivoire (1977-1987), Morocco (1985-1990), Venezuela (1983-1988), and Mexico (1984-1990).	U.S. PAOC by sector; or Pollution intensity of sectors based on IPPS	Host country capital-labor ratio, concentration, import penetration, market size, and FDI barrier dummy by sector. Source country wages by sector.	PAOC coefficient is insignificant. Pollution intensity coefficient is also insignificant. But the latter data does not vary over time, raising questions about its role in a fixed effects model.
	Cross-section, ordinary least squares	Energy intensity of plants.	N/A	Plant characteristics including foreign vs. domestic, size, capital intensity, age, R&D, and electricity price.	Foreign plants have lower energy intensity than comparable domestic plants in these countries.
	Panel, fixed effects	Outbound U.S. investment by sector.	U.S. PAOC by sector	Sector and time dummies, skilled labor, capital, R&D, and export intensity.	Does not control for sample selection. PAOC has an insignificant effect on outbound U.S. investment.

(continued)

Table 5 (continued)

Study	*Empirical Approach*	*Dependent Variable*	*Environmental Variable*	*Control Variables*	*Findings (and critiques)*
List and Kunce (2000)	Panel, sample selection model	Change in manufacturing employment in United States, 1982-1994.	N/A	State-level population density, education, unionization, energy, taxes, highway miles, fixed effects, and time dummies.	Employment levels are positively affected by education and negatively affected by unions and energy costs.
	Cross-section, ordinary least squares	Average state residuals from above.	State regulatory spending in 1986, or PAOC in 1982.	Time-invariant state attributes such as land area, (land area)2, right-to-work dummy.	Environmental variables are insignificant if you pool all manufacturing industries. However, they are *negative* and significant for dirtier industries.
Clark, Marchese, and Zarrilli (2000)	Cross-section, logit	Decision to conduct offshore assembly in LDCs, by U.S. 4-digit sectors, 1992.	Toxic intensity of sector's output.	U.S. sector attributes such as labor intensity, tariff rates, nontariff barrier dummy, import penetration, and transportation costs.	Toxic intensity of sector output has highly significant negative effect on likelihood of engaging in offshore assembly.

Berman and Bui (2001)	Treatment vs. comparison groups	Change in employment in refineries in Los Angeles, 1979-1992.	Direct measures of regulations, such as date of adoption, compliance, and increased stringency.	Industry and region dummies.	No evidence that local air quality regulations reduces labor demand. But refineries are capital intensive.
Smarzynska and Wei (2001)	Cross-section, probit with clustering	Probability of inbound FDI at firm level in 24 transition economies, 1995.	Proxy for firm pollution intensity: U.S. sectoral [emissions / sales], or [(PACE + PAOC) / sales]. Proxy for host country standards: participation in international treaties, NGOs per million people, observed emissions, or EBRD index.	Firm characteristics such as size, R&D intensity, and regional experience. Host country characteristics, such as labor costs, corporate tax rate, corruption index, and distance.	Coefficient of interaction between emissions and treaty participation is negative and significant. Result does not survive robustness check using other proxies for pollution intensity and host country standards. Also, emissions may not be exogenously determined.
Greenstone (2002)	Panel, fixed effects	Change in employment, output, and investment in U.S. manufacturing plants, 1972-1987.	County ambient air quality standard attainment status.	Plant, industry, county and period effects.	Nonattainment status has *negative* effect on employment, investment and output.

(continued)

Table 5 (continued)

Study	Empirical Approach	Dependent Variable	Environmental Variable	Control Variables	Findings (and critiques)
Keller and Levinson (2002)	Panel, fixed effects	Value of FDI into United States, 1977-1994.	PAOC / value added, adjusted for states' industrial composition.	Time dummies, state characteristics including dummies, energy costs, land prices, wages, and unionization.	Environmental coefficient is negative and significant. However, magnitude is economically small.
	Panel, count data model	No. of planned foreign plants in United States, 1977-1994.	Same as above	Same as above	Same as above
Xing and Kolstad (2002)	Cross-section, ordinary least squares	Outbound U.S. FDI to 22 countries in 6 separate manufacturing sectors, 1985, 1990.	Observed SO2 emissions in host country.	Host country characteristics such as GDP, share of industry in GDP, electricity source, and corporate tax rate.	Observed host country emissions have an insignificant effect on FDI. OLS is biased.
	Cross-section, instrument variable model	Same as above	Instruments for SO2 including infant-mortality rate, population density, and all control variables.	Same as above	Predicted host country emissions have a significant *negative* effect on FDI.

Notes: FDI = foreign direct investment; PACE = pollution abatement capital expenditures; PAOC = pollution abatement operating costs; LDC = less developed country; NGO = nongovernmental organizations; EBRD = European Board for Reconstruction and Development; SO_2 = sulfur dioxide.
Studies that find evidence of a significant pollution haven effect are highlighted by the use of *italics* in the last column of this table.

Discussion and Conclusions

At the crux of the pollution haven debate is the fear that trade liberalization will induce a race to the bottom as regions compete for industry and jobs by easing environmental standards and regulations. This concern implicitly assumes that capital and goods flows respond to regional regulatory differences. As shown in the literature section, much of the empirical literature that has attempted to test this assumption has arrived at differing conclusions, ranging from a modest deterrent effect of environmental regulatory stringency on economic activity to a counterintuitive modest attractive effect. In this section, we highlight the lessons learned about the different methodologies and data sources, and we present concluding remarks.

STUDY DESIGN

The results of the various pollution haven studies are likely to be driven by their different underlying assumptions and methodologies and are therefore not easily comparable. Indeed, the results are not comparable even for studies using the same methodology (e.g., conditional logit) because each study uses a different sample, different measures of regulatory stringency, and a different set of independent variables.

Dependent Variable

Researchers have used various measures of economic activity ranging from plant births to inbound and outbound foreign direct investment to net exports. One might be tempted to attribute the contradictory results found in the literature to this difference in dependent variable. For example, Xing & Kolstad (2002) argued that capital flow (investment) will be more affected by differential environmental regulations than goods flow (net exports) because the production mix will only change in the long run. However, we find that the choice of dependent variable has a smaller effect on the researcher's ability to detect industrial flight patterns than the choice of methodology (e.g., panel vs. cross-section). For example, Becker and Henderson (2000) and Levinson and Taylor (2003) used panel data and found evidence of a pollution haven effect even though the former study focuses on plant births and the latter on net exports.

Regulatory Stringency Measure

As shown in column 4 of Tables 3, 4, and 5, regulatory stringency has also been proxied in a variety of ways in the literature. Some of these measures have obvious drawbacks. For example, environmental indices are easy targets for criticism because of their subjectiveness. Similarly, studies that use IPPS coefficients to estimate foreign emissions invariably find no evidence of pollution haven effects, possibly because they assume that each industry's pollution characteristics are the same across all countries and time periods. We find studies that use objective, quantitative data on pollution levels or pollution costs more convincing. County ambient air quality standard attainment status is based on monitoring data. Industry or firm emissions data may be self-reported, but they are also subject to inspection. Similarly, although abatement cost data can over- or understate true costs, Brunnermeier and Cohen (2003) noted that the difference is arguably not significant.

Control Variables

The various control variables used in the literature are summarized in column 5 of Tables 3, 4, and 5. Intuition suggests—and the empirical literature verifies—that firms are attracted to regions with a larger market size because they offer better infrastructure, agglomeration economies, and access to consumer markets. Unfortunately, the literature provides no consensus estimates of the sign and magnitude of control variables other than market size. For example, although theory predicts that firms will be attracted to states offering cheap labor, studies report negative and positive coefficients on wages. The positive coefficients most likely arise because some studies fail to control for productivity and skill of labor and because increased economic activity has a feedback effect on wages. We would view with suspicion the results of models that do a poor job of predicting the signs of control variables. For example, although the coefficient of the environmental variable is insignificant in Tobey (1990), so are the coefficients of most of the resource endowment variables.

Geographical Unit of Analysis

Manufacturing plants may find it easier to relocate within a country than to cross national borders because of the smaller disparity in infrastructure, labor skills, and transportation costs. However, differences in the stringency of environmental regulations are also presumably smaller within countries than across international boundaries, so these factors could cancel out. Our review suggests that empirical findings of pollution haven effects depend more on the estimation methodology (e.g., panel vs. cross-section, ordinary least squares vs. two-stage least squares) than on the geographic unit of analysis of the study.

Level of Industry Aggregation

The selection of industry sample may affect the results of the analysis. Some researchers (for example, Bartik, 1988; Friedman et al., 1992) pool together all industries, but this may mask pollution haven effects in specific industries. A similar problem may be caused by the aggregation of establishment-level data to a coarser level. Other researchers (e.g., Tobey, 1990; Low & Yeats, 1992) only examined dirty industries. However, dirty industries might share unobservable characteristics (such as natural resource intensiveness) that also make them immobile. By restricting the sample to dirty industries, one might unwittingly select the least geographically footloose industries. Limiting the sample to pollution-intensive industries also throws out an important source of variation. We would like to see not only whether pollution regulations increase net imports of pollution industries but also whether this effect is larger than (or even of the opposite sign to) the effect of pollution regulations on imports of clean industries.

Empirical Methodology: Cross-Section Versus Panel Data

Cross-sectional studies tend to reject the pollution haven effect. Some of these studies even find a counterintuitive sign on the environmental variable (e.g., Grossman and Krueger, 1993; Mani et al., 1996), that is, they find that economic activity is attracted to jurisdictions with stricter environmental regulations. For most of these studies, however, the environmental coefficient is statistically and economically insignificant.

In contrast, recent studies using panel data typically find some evidence of a moderate pollution haven effect. This effect has been noted at the state or county level using plant data (Henderson, 1996; Becker & Henderson, 2000; List & Kunce, 2000; Greenstone, 2002; and Keller & Levinson, 2002) and at the industry level using net export data (Ederington et al., 2003; Ederington & Minier, 2003; and Levinson & Taylor, 2003). These findings highlight the importance of controlling for unobserved heterogeneity.

Empirical Methodology: Endogeneity Correction

Studies that adjust the observed measure of regulatory stringency for industrial composition or endogeneity tend to find more robust evidence of a moderate pollution haven effect. For example, Ederington and Minier (2003) and Levinson and Taylor (2003) found that U.S. net exports are not affected by abatement costs when the latter are treated as exogenous but are significantly affected when these costs are treated as an endogenous variable. However, these coefficients are all of an economically small magnitude. In addition, as is always true of instrumental variables analyses, the instruments are open to critique.

CONCLUDING REMARKS

The early literature based on cross-sectional analyses typically tended to find that environmental regulations had an insignificant effect on firm location decisions. However, several recent studies that use panel data to control for unobserved heterogeneity, or instruments to control for endogeneity, do find statistically significant pollution haven effects of reasonable magnitude. Furthermore, it does not appear to matter whether these studies look across countries, industries, states, or counties, or whether they examine plant location, investment, or international trade patterns. When enough data is available, a metadata analysis could be conducted to test some of our conclusions more directly.

The existing studies in the literature largely represent exercises in positive or descriptive economics. These studies can only tell us whether capital and goods flow are sensitive to regional differences in environmental regulations. It is impossible to draw normative or policy conclusions based on these results alone, that is, the finding that firms are responsive to regulatory differences in their location decisions does not demonstrate that governments purposely set suboptimal environmental regulations to attract business. Indeed, it may be efficient for polluting industries to move to regions that put less emphasis on environmental quality, provided they do so for appropriate reasons (i.e., there is no market failure, political failure, or redistributional concern involved). That issue, however, is the subject of a separate literature.[5]

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