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FOOTLOOSE AND POLLUTION-FREE

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ABSTRACT

In numerous studies, economists have found little empirical evidence that environmental regulations affect trade flows. In this paper, we propose and test several common explanations for why the effect of environmental regulations on trade may be difficult to detect. We demonstrate that while most trade occurs among industrialized economies, environmental regulations have stronger effects on trade between industrialized and developing economies. We find that for most industries, pollution abatement costs are a small component of total costs, and are unrelated to trade flows. In addition, we show that those industries with the largest pollution abatement costs also happen to be the least geographically mobile, or "footloose." After accounting for these distinctions, we measure a significant effect of pollution abatement costs on imports from developing countries, and in pollution-intensive, footloose industries.

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1. Introduction

Conventional wisdom in the U.S. is that environmental regulations have diminished the ability of U.S. manufacturers to compete internationally, and thus have contributed to the relocation of the U.S. manufacturing sector overseas and to the growing U.S. trade deficit. Discussion has centered on the extent to which environmental regulations have imposed significant costs on pollution-intensive industries located in the U.S., and the extent to which these regulations have caused pollution-intensive industries to migrate to less regulated countries (the so-called "pollution haven hypothesis"). The argument that stringent environmental regulations could affect comparative advantage, altering international patterns of trade, is fairly intuitive and has considerable theoretical support.[1] However, there has been little empirical support for the proposition that environmental regulations affect trade. In a survey article, Jaffe, Peterson, Portney and Stavins (1995) conclude that while environmental regulations do impose large and significant costs on polluting industries, these costs have not appreciably affected patterns of international trade.

Given that the United States is the only country that has collected pollution abatement cost data for a significant period of time, researchers have limited options for exploring the relationship between environmental regulations and competitiveness. Previous studies have either taken the approach of examining the effects of environmental controls on U.S. net imports (e.g., see Kalt (1988) and Grossman and Krueger (1993)), or examining international trade patterns by relying on qualitative measures of regulatory stringency in different countries (e.g., see Tobey (1977)). Neither of these methods has resulted in quantitatively significant or robust evidence that environmental regulations influence trade patterns. However, given the underlying logic of the pollution haven hypothesis, researchers continue to attempt to explain why effects of environmental regulation on

[1]See Pethig (1976), Siebert (1977), McGuire (1982), and Copeland and Taylor (1994).

competitiveness are so difficult to detect.[2] In this paper we provide and test several candidate explanations for the lack of evidence on the pollution haven hypothesis. These explanations share the assumption that there is underlying heterogeneity in the relationship between environmental regulations and trade flows that has been overlooked in previous research.

Our first candidate explanation is that most trade takes place among developed countries, which share similarly high levels of environmental stringency.[3] As a result, the U.S. imports relatively more from countries with relatively stringent regulations, a seeming violation of the pollution haven hypothesis. Empirical work that aggregates trade flows across multiple countries may mask significant effects of environmental costs for countries with distinct patterns of regulation.

Our second hypothesis is that some industries are less geographically mobile, due to transportation costs, plant fixed costs, or agglomeration economies. Consequently, these less mobile industries will be insensitive to differences in regulatory stringency between countries because they are unable to relocate easily. Cross-industry regressions that average over multiple industries could conceal the effect of environmental regulations on trade in the more "footloose" industries.

Finally, our third candidate explanation is that, for all but the most heavily regulated industries, environmental regulation represents only a small portion of total production costs. Therefore, for the majority of industries, the effect of differences in these small costs is overwhelmed by differences in the prices of more important factors, and by noise in the data. Once again, empirical approaches that average over multiple industries could mask the fact that

[2] For example, Ederington and Minier (2003) and Levinson (2003) argue that previous research has found little evidence for the pollution haven hypothesis because it treats the level of environmental regulation as an exogenous variable. Using instrumental variables, they find statistically significant, economically meaningful negative effects of environmental regulations on economic activities when the level of environmental regulation is treated as endogenous.

[3] In addition, even when there are substantial differences in regulations across countries, these differences may be temporary given the tendency for convergence in environmental standards across countries. Thus, given the costs of relocation, foresighted industries may not pursue the short-term gains from locating in less regulated areas.

environmental regulations do affect trade in those industries where environmental costs are significant. Moreover, the most polluting industries may be the least footloose, making the pollution haven effect particularly difficult to detect. In the following sections, we test each of these explanations in turn.

2. Baseline empirical specification

The only country that has collected pollution abatement cost data for a significant time period is the United States, in the form of the Pollution Abatement Costs and Expenditures (PACE) survey, which publishes manufacturers' pollution abatement costs at the 4-digit industry level. Because the PACE pertains to U.S. manufacturers, the only way to use these data to estimate the effects of environmental regulations on trade is to compare imports and exports from the U.S. as a function of industry characteristics. This is the methodology employed by Grossman and Krueger (1991) in a cross section, and by Ederington and Minier (2003) and Levinson (2003) exploiting the panel data. In this paper we use a panel data set, constructed by Ederington and Minier (2003) which includes, at the 4-digit SIC level, pollution abatement operating costs and a vector of industry characteristics for the years 1978-92.[4]

Following the previous literature, we regress net imports by industry i in year t (M_{it}) on the industry's environmental costs (E_{it}), trade barriers (τ_{it}), and a vector of factor intensity variables (F_{it}^{n}), as well as industry and time-specific fixed effects (a_i and a_t):[5]

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[4] We update the Ederington and Minier (2003) dataset by using the recently revised Feenstra (1996,1997) dataset on industry trade flows and the NBER-CES Manufacturing Industry Database of Bartelsman, Becker and Gray on industry characteristics.

[5] While trade economists recognize that a cross-industry regression of trade flows on factor intensities is not a valid test of the Heckscher-Ohlin model of international trade, our motivation for including factor intensity variables in the regression is simply to act as industry controls to better address the relationship between environmental regulations and trade flows.

$$M_{it} = a_i + a_t + b_1 E_{it} + b_2 \tau_{it} + \beta F_{it}^{n} + \varepsilon_{it} \qquad (1)$$

The dependent variable (M_{it}) is net import penetration: U.S. imports minus exports scaled by total U.S. shipments in industry i at time t. The stringency of environmental regulations (E_{it}) is measured by the ratio of pollution abatement costs to total costs of materials, while τ is estimated by dividing duties paid by total import volume to give a measure of average ad valorem tariffs. The factor intensity variables measure the human and physical capital intensity of each industry. To calculate the (direct) factor shares of both types of capital, we follow a procedure suggested by Grossman and Krueger (1991) in which the payroll expenses of an industry are divided into payments to unskilled labor and human capital and then scaled by value added. The remaining portion of value added is assumed to be payments to physical capital. We discuss these variables in more detail in Appendix A, and descriptive statistics for these variables appear in the first column of table 1.

For comparison to previous empirical work, we begin by estimating equation (1), with year and industry fixed effects.[6] Here the estimated coefficient on environmental costs (0.20) is small and statistically insignificant. The other coefficient estimates in table 1 are as expected: both human and physical capital are sources of comparative advantage for the U.S. (indicated by negative coefficient estimates), and higher tariffs are correlated with lower net imports. To understand the magnitude of the estimated effect of environmental costs, consider it in elasticity terms. Evaluated at the means of the environmental cost and net import variables, the implied elasticity is about 0.04. A 20 percent increase in the environmental costs faced by an industry, relative to other industries, is associated with less than a one percent increase in net import penetration in that industry.

[6] During the empirical estimation we discovered that our import regression was sensitive to the inclusion of outlying observations. We used an approach suggested by Hadi (1992, 1994) to identify outliers in our dataset; these eight outliers (0.2% of the full sample) were excluded from the analysis. See Appendix A for details.

As is typical in the empirical literature, simple correlations between net imports and environmental regulations fail to uncover a strong relationship. However, table 2 presents an estimate of the *average* effect of environmental regulations on *total* trade flows between the U.S. and all other countries, for all industries. We may be missing some important underlying heterogeneity across industries or countries in the relationship between regulatory stringency and competitiveness. In the following sections, we discuss and test several theories of the possible sources of such heterogeneity.

3. Trade with high and low-standard countries

The first hypothesis we investigate is whether similarities (or expected convergence) in environmental standards across countries obscure the relationship between environmental regulations and trade flows. Specifically, most of the world's trade volume occurs among developed countries, which have similar levels of environmental standards. Consequently, the U.S. imports relatively more from countries with relatively stringent regulations. Moreover, if differences in regulations between developed countries are perceived as temporary, then given the costs of relocation, industries may not pursue the short-term gains from locating in temporarily less stringently regulated areas.

As a test of this hypothesis, we reconstruct the data by dividing trade flows in each industry into trade with countries with high environmental standards (i.e., similar to the U.S.), and those with low environmental standards; we also use high- and low-income countries to proxy for differences in environmental standards. The idea is that an increase in U.S. environmental standards will have a greater effect on U.S. trade with low-standard countries than with other high-standard countries. The reason for this is two-fold. First, during the time period of our empirical analysis (1978-92), an increase in U.S. environmental regulations was less likely to be matched by a comparable increase in environmental regulations in countries with low environmental standards. Second, even if firms believed the increase in U.S. environmental regulations would eventually be matched in the future by regulatory increases in other countries, the time horizon for that convergence is likely to be much longer in the low-standard country, making firms more likely to pursue the gains to relocating to the low-standard country.

We use two different methods of dividing our sample into trade with high and low-standard countries. First, in columns (1) and (2) of table 2, we divide the trading partners of the U.S. into OECD and non-OECD countries under the assumption that OECD countries have environmental standards more comparable to the U.S. than do non-OECD countries. Note that the explanatory variables for each industry are identical in the two regressions (and identical to the panel regression of column (2) of table 1). The difference is that the dependent variable is net imports to OECD countries in column (1) and net imports to non-OECD countries in column (2). Second, we divide trade based on an environmental stringency ranking provided by Eliste and Fredriksson (2002) which is based on the rankings of Dasgupta et.al. (1995); these results appear in columns (3) and (4).

The environmental stringency index in Eliste and Fredriksson (2002) covers 61 countries for agricultural industries and 30 countries for manufacturing industries. Since the correlation coefficient between agricultural and manufacturing stringency is 0.96 for the 30 countries with data on both, we use the agricultural index to maximize country coverage. The ranking ranges from 49 to 186: the U.S. rank is 186 and the median is 92. We divided the sample between 117 (South Korea) and 133 (Greece), which is the largest break in the data. This gives us 20 countries in the high environmental standards sample, and 33 countries in the low-standard sample.[7] Again, the

[7] Trade with 43 countries is omitted from this division due to missing data on environmental standards for these trading partners.

dependent variable is net imports from these countries in regressions 3 and 4 respectively; the explanatory variables are identical for each industry-year observation.

For each sample we estimate equation (1). In both cases, the results support our interpretation. Specifically, while the coefficient on environmental costs is negative (and not statistically significant) for trade with the OECD countries, it is positive (and statistically significant) for the non-OECD countries.[8] Intuitively, while an increase in U.S. environmental costs will not have a significant impact on trade with other OECD countries, it will lead to a statistically significant increase in net imports from developing countries. In addition, while the coefficient estimate on environmental costs for non-OECD countries (0.25) is comparable in magnitude to that for the full sample (0.20), this implies a larger, more quantitatively significant effect since trade volume is lower than in the full sample. Specifically, evaluated at the means of environmental costs and net imports (scaled by industry size), the implied elasticity is about 0.2 for trade with non-OECD countries (about five times greater than the elasticity for the full sample). Thus, we do find evidence that estimating the average effect of an increase in environmental costs over all trade understates the effect such an increase in regulatory stringency has on trade with low income or low standard countries.

4. "Footloose" industries

The second hypothesis that we investigate is whether the relationship between environmental regulations and trade flows is obscured because pollution-intensive industries tend to be less geographically mobile, or "footloose," than other industries. As is common in the empirical literature on trade and the environment, in section 2 we estimated the *average* effect of an increase in

[8] Perhaps not surprisingly, our results suggest that human and physical capital are sources of comparative advantage for the U.S. only with respect to trade with low-income countries (indicated by negative coefficient estimates).

environmental regulation on net imports across U.S. manufacturing industries. However, this approach ignores the fact that an increase in environmental costs will likely have different effects on different industries. Some industries (because of high transport or relocation costs) may be insensitive to changing comparative advantage or changes in production cost, while other industries (the footloose industries) are more sensitive. Cross-industry regressions that find little average effect could conceal the relationship in more mobile industries. In what follows we explore three potential determinants of geographic immobility: transportation costs in product markets, plant fixed costs, and agglomeration economies. Complete definitions, data sources, and descriptive statistics appear in Appendix A.

Our first measure of industry mobility is the product-market transport costs of an industry. Consider a high-transport-cost industry, such as cement (SIC 324). Even a large increase in environmental costs will not significantly affect cement trade flows, because transport costs prevent cement manufacturers from locating far from customers. By contrast, a low-transport-cost industry can more freely relocate and will be more sensitive to environmental cost changes. Thus our hypothesis is that an increase in environmental costs will have a greater effect on net imports in industries with low transport costs. We estimate the product market transportation costs for each industry by using freight costs controlling for the distance shipped.[9]

Our second measure of immobility is the fixed plant costs of an industry. Consider an industry with significant plant costs, such as building, paper and board mills (SIC 266). Such an industry would be less likely to relocate or change jurisdictions because the relocation would incur significant costs, specifically the sinking of a large amount of investment into a plant in the new jurisdiction. Industries with large fixed costs may be less sensitive to increases in environmental

[9] Specifically, we use the fixed effects coefficients from a regression of transport costs on distance and distance squared for the 15 largest trading partners of the U.S.; for details see Appendix A.

costs, since the costs of relocation might outweigh the gains to locating in a less stringent jurisdiction, especially if differences in environmental regulations between jurisdictions were viewed as temporary. Alternatively, an industry with few fixed costs might aggressively pursue even temporary sources of comparative advantage since the costs of relocation are smaller. Thus our hypothesis is that an increase in environmental costs will have a greater effect on net imports in industries with low plant costs. As a measure of fixed plant costs, we use data from the NBER-CES Manufacturing Industry Database of Bartelsman, Becker and Gray on real capital structures in an industry.

Our third measure of immobility is the extent of agglomeration economies of an industry. While the sources of agglomeration economies are varied (e.g., knowledge spillovers, labor market pooling), the effect is that firms will have an incentive to locate near one another. Consider an industry with significant agglomeration economies, such as SIC 227, floor covering mills. Such an industry may be insensitive to changes in environmental costs if the gain to remaining close to other firms in the industry outweighs the gain from relocating to a less regulated jurisdiction.[10] This reasoning parallels that commonly given to explain how patterns of specialization can persist in international trade even as relative production costs change over time. Thus our hypothesis is that an increase in environmental costs will have a larger effect on net imports in industries with small external economies. To estimate the extent of external economies in an industry, we use an index of geographic concentration of U.S. manufacturing industries from Ellison and Glaeser (1997).[11]

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[10] Note that external economies of scale in an industry could lead to a situation where it would be in the industry's best interest to change jurisdictions, but not in any firm's individual interest to do so unilaterally.

[11] Note that since the Ellison and Glaeser (1997) index is based on geographic concentration, it is not a pure measure of agglomeration economies and thus industrial immobility. Specifically, very mobile firms migrating to an area of comparative advantage could also lead to a geographically concentrated industry, and such an industry would remain sensitive to changes in environmental regulations.

The results are in table 3, where we add interaction terms between environmental costs and these three measures of immobility, as calculated as in Appendix A, to equation (1). If our hypotheses are correct, these interactive terms will have negative coefficients, indicating that changes in environmental costs only have large effects on trade flows in more footloose industries. In column (1), the measure of industry immobility is (distance-controlled) transport costs, and the interaction term is negative and statistically significant. Evaluated at the average transport costs for an industry (0.009), this implies a coefficient estimate on environmental costs of 0.17, which is very similar to that computed in the base regression of Table 1.[12] In addition, the negative coefficient on the interactive term implies that, as predicted, industries with above average transport costs will be less sensitive to changes in environmental costs.

Column (2) of table 3 repeats the analysis of column (1), but with plant fixed costs as the measure of geographic immobility. In this case, the coefficient estimate for an industry with average plant costs (0.237) is higher than that of the base regression of table 1 (a coefficient estimate of 0.82 rather than 0.20). However, as predicted, the negative coefficient on this interactive term implies that industries with plant costs above (below) average will be less (more) sensitive to changes in environmental regulations (and this difference is statistically significant). In column (3) of table 3 we use agglomeration economies as our measure of industry immobility. Evaluated at the average degree of agglomeration for an industry (0.051), this implies a coefficient estimate on environmental costs of 0.22, similar to that calculated in the base regression of Table 1. As in the previous regressions, the negative coefficient estimate on the interactive term implies that this coefficient estimate will be higher for industries with below average agglomeration economies, although in this

[12] Note that our measure of transport costs is a fixed-effect coefficient, and thus is roughly centered around zero, with positive measures implying industries with above average transport costs and negative measures implying industries with lower than average transport costs.

case the interactive term is not statistically significant. In all three regressions we find support for our hypotheses.

To compare the quantitative significance of these results, column (4) repeats the analysis including all three measures of industry immobility. As can be seen in column (4), the interactive term on plant costs is the only interactive term that remains statistically significant. (It is also the most quantitatively significant, as it explains the majority of the sensitivity differences across industries in the analysis that follows). Our results suggest that, for an industry which has the median level of all three immobility measures, an increase in environmental costs of one percentage point would result in a decrease in net imports of 0.96 percentage points. Evaluated at the mean of both environmental cost and net imports, this results in an implied elasticity of about 0.2. In contrast, in a less mobile industry (in the top 20^{th} percentile of all three measures of industry immobility), the same increase in environmental costs would result in a decrease in net imports of only 0.2 percentage points (an implied elasticity of only 0.04). Likewise, in a more mobile industry (in the bottom 20^{th} percentile of all three immobility measures), the same increase in environmental costs would decrease net imports by 1.5 percentage points (an implied elasticity of 0.32, which is 8 times greater than that for the top 20^{th} percentile). We interpret this as evidence that estimating the average effect of an increase in environmental costs over all industries understates the effect such an increase in regulatory stringency has on trade in the more footloose industries.

5. Small environmental costs

The final hypothesis that we investigate is whether environmental regulations have little effect on measures of industrial competitiveness because, for all but the most heavily regulated industries, the costs of compliance with U.S. environmental regulation make up a relatively small portion of total production costs. In our dataset, environmental costs average around one percent of total material costs. Thus, the stringency of environmental regulations may not be a significant determinant of comparative advantage for most U.S. industries, since it may be dwarfed by other determinants of industry location, such as labor costs or infrastructure. However, environmental costs do comprise a large share of total cost for a few pollution-intensive industries (chemical manufacturing, petroleum, primary metals, etc.). Environmental regulatory stringency may be a significant determinant of net imports in these more pollution-intensive industries, and cross-industry regressions that estimate the average effect may obscure the effect in high-cost industries.

To test this hypothesis, we compute the average of environmental costs for each industry over 1978-92 as a measure of the importance of environmental regulation in that industry. We then estimate a version of equation (1) in which we include the interaction between the *average* environmental costs in an industry, and the current level in any year. If more polluting industries are more sensitive to environmental cost increases, we expect the coefficient on this interactive term to be positive. Instead, the coefficient in table 4 (-31.13) is negative, although only statistically significant at the 90% level. This result suggests that the effect of an increase in environmental costs is actually smaller in the more pollution-intensive industries.

One explanation for why industries with large average pollution abatement costs may be less sensitive to increases in those abatement costs over time is because the more pollution-intensive industries also may be less footloose. To test this hypothesis, in column (2) of table 4 we included both an interactive term for average pollution abatement costs and the interactive terms for our three immobility measures. While the coefficient estimates for our three immobility measures are largely unchanged from table 3, the coefficient estimate on average pollution abatement costs is much smaller than in regression (1) of table 4 (-3.6) and not at all statistically significant. This result lends credence to the argument that one reason for the lack of empirical evidence for the pollution haven hypothesis is the lack of geographic mobility on the part of the more pollution intensive industries.

6. Conclusion

The lack of empirical support for the proposition that environmental costs affect trade flows has been a puzzle in the trade and environment literature. In this paper, we propose and test three explanations for why previous research may have failed to find any robust relationship between environmental regulations and trade flows. We find support for two explanations. First, we find that estimating the average effect of an increase in environmental costs over all trade flows understates the effect of environmental regulations on trade with low-income or low-standard countries. Second, we find that estimating the average effect of an increase in environmental costs over all industries understates the effect that regulatory stringency has on trade within the geographically mobile (i.e., footloose) industries. Importantly, polluting industries also appear to be relatively immobile. Failing to take account of this correlation can give the counter-intuitive finding that polluting industries are less sensitive to increases in environmental costs.

We find no evidence for our third hypothesis, that trade flows are more sensitive to changing environmental regulations in the more pollution-intensive industries (where environmental costs are a greater percentage of total costs). In a way, the lack of support for this hypothesis is also a noteworthy finding, as the argument that environmental costs are simply too small in most industries to appreciably affect industry location is one of the most common arguments advanced for the lack of empirical evidence for the pollution haven hypothesis. Indeed, this is typically the explanation that is given both in survey articles (see, e.g., Jaffe, et. al (1995) and Levinson (1996)) and in more mainstream discussion of the trade-environmental relationship. However, we find little relationship between the stringency of environmental regulations in an industry and the sensitivity of that industry to changes in environmental costs.

In summary, our results suggest that in predicting the effects of environmental regulations on industries, it is important to account for these industry characteristics: the amount of trade with low income countries and the geographic mobility of the industry. And while this paper focuses on the effects of environmental regulations, the intuition behind the results applies to any regulatory change. It would be an interesting topic of future work to see if the same patterns exist for other regulations such as health and safety standards or labor regulations.

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Table 1: Means and Baseline Regression

	Means (s.e.) (1)	Baseline (2)
Dependent variable:	0.051	
net imports / value shipped	(0.279)	
Environmental cost	0.011	0.20
	(0.014)	(0.27)
Tariff	0.046	-0.37*
	(0.073)	(0.05)
Human capital	0.230	-0.30*
	(0.091)	(0.14)
Physical capital	0.605	-0.16
	(0.123)	(0.10)
Observations	3,818	3,818
Number of industries	382	382

Notes to Table: The regression is estimated with year and industry fixed effects, and covers the period 1978-92 (1979 and 1987 are omitted due to missing data). * indicates statistical significance at the 5% level.

Table 2: Trading partners' environmental standards

	OECD (1)	Non-OECD (2)	High-standard (3)	Low-standard (4)
Environmental cost	-0.22	0.25*	-0.23	0.11
	(0.15)	(0.10)	(0.15)	(0.07)
Tariff	-0.02	-0.13*	-0.01	-0.05*
	(0.03)	(0.02)	(0.03)	(0.01)
Human capital	0.11	-0.25*	0.11	-0.20*
	(0.08)	(0.05)	(0.08)	(0.04)
Physical capital	0.12*	-0.15*	0.12*	-0.12*
	(0.06)	(0.04)	(0.06)	(0.03)
Observations	3,818	3,816	3,818	3,815
Number of industries	382	382	382	382

Notes to Table: The dependent variable in each regression is net imports weighted by value shipped to specified trading partners (OECD countries in regression 1, non-OECD in regression 2, countries with high environmental standards in regression 3 and low standards in regression 4). All regressions include year and industry fixed effects.

* indicates statistical significance at the 5% level.

Table 3: “Footlooseness”

	Transport costs (1)	Plant costs (2)	Agglomeration (3)	All three (4)
Environmental cost	0.30	2.12*	0.29	1.99*
	(0.27)	(0.54)	(0.33)	(0.55)
Tariff	-0.37*	-0.37*	-0.37*	-0.37*
	(0.05)	(0.05)	(0.05)	(0.05)
Human capital	-0.31*	-0.30*	-0.30*	-0.31*
	(0.14)	(0.14)	(0.15)	(0.14)
Physical capital	-0.16	-0.16	-0.15	-0.16
	(0.10)	(0.10)	(0.10)	(0.10)
Interaction terms:				
Transport costs ×	-14.69*			-12.31
environmental cost	(7.37)			(7.89)
Plant costs ×		-5.47*		-5.39*
environmental cost		(1.33)		(1.37)
Agglomeration			-1.35	2.84
economies ×			(2.87)	(3.10)
environmental cost				
Observations	3,818	3,818	3,818	3,818
Number of industries	382	382	382	382

Notes to Table: The dependent variable in each regression is net imports weighted by value shipped. Regressions are estimated with year and industry fixed effects.
* indicates statistical significance at the 5% level.

Table 4: Pollution Intensity

	(1)	(2)
Environmental cost	1.15*	2.05*
	(0.58)	(0.65)
Industry average environmental cost × environmental cost	-31.13	-3.60
	(16.76)	(18.42)
Tariff	-0.37*	-0.37*
	(0.05)	(0.05)
Human capital	-0.31*	-0.31*
	(0.15)	(0.14)
Physical capital	-0.16	-0.16
	(0.10)	(0.10)
Transport costs × environmental cost		-12.28
		(7.90)
Plant costs × environmental cost		-5.28*
		(1.48)
Agglomeration economies × environmental cost		2.88
		(3.11)
Observations	3,818	3,818
Number of industries	382	382

Notes to Table: The dependent variable is net imports weighted by value shipped. The regressions are estimated with year and industry fixed effects.

* indicates statistical significance at the 5% level or better.

Appendix A: Data

Omitted Outliers:
Because the regressions were highly sensitive to several outlying observations, we performed the analysis of Hadi (1992, 1994), which identified outlying observations in three industries.
Industry 3489 (ordnance and accessories) is identified as an outlier for years after 1987, due to what appears to be an error in the concordance (its environmental costs jump significantly post-1987, to as high as 62% of total costs in 1991).
Industry 3263 (fine earthenware food utensils) has non-missing data on environmental cost only in 1985 and 1986; it is identified as an outlier due primarily to very high levels of net imports in those years (9.0 and 14.2, relative to a sample mean of 0.05).
Industry 3332 (primary lead) in 1981 is an outlier for the human and physical capital variables (6.1 and -8.1, respectively, relative to sample means of 0.2 and 0.6).
We omitted the above 8 observations from the original sample of 3,826.

Transport Costs:
To compute transport costs, we used data at the industry level by country of export, for the 15 largest exporters to the U.S. in 1990 (Canada, Japan, Mexico, Germany, Taiwan, United Kingdom, Republic of Korea, China, France, Italy, Saudi Arabia, Singapore, Hong Kong, Venezuela, and Brazil). At the 10-digit HS code level, we downloaded import data from each of these countries to the U.S., summing over all ports of entry. At this level of disaggregation, the data include both the customs value and the CIF value of imports; total transport costs are the difference between these as a percentage of CIF value. We aggregated data from the HS level to the MSIC level (provided in data set). For 1988-92, we converted the data from 1987-MSIC to 1972-MSIC using a concordance from the Feenstra CD-rom. Then all data were converted from 1972-MSIC to 1972-SIC using a concordance from Chris Magee.

To estimate transport costs controlling for distance, we ran a fixed-effects panel regression of these estimated transport costs on distance and distance squared, including time and industry fixed effects (distance is the great circle distance between country capitals, from Jon Haveman's website). Specifically, we estimate

$$C_{ijt} = \alpha_1 D_j + \alpha_2 D_j^2 + \sum_t \beta_t I_t + \sum_i \delta_i I_i$$

where C_{ijt} represents transport costs as a percent of the CIF value of imports for industry i from country j in year t, D is the distance between country j and the U.S., I_t is an indicator variable equal to one in year t, and I_i is an indicator variable equal to one for industry i. Our measure of distance-controlled transport costs for each industry is the coefficient δ_i.

Plant Fixed Costs:
Our measure of plant fixed costs is taken from Bartelsman, Becker and Gray ("The NBER Manufacturing Productivity Database," NBER Technical Paper 205, as updated on website), and is defined as real structures capital stock. We scale this by industry shipments; the data are provided at the 1972-SIC level. We use industry means over the observations included in the sample (i.e., data not included in the 3,818 observation sample are not used to compute the means).

Agglomeration Economies:
To measure agglomeration economies, we use the index of geographic concentration proposed by Ellison and Glaeser (1997). This measures deviations from randomly distributed employment patterns (γ, their measure, equals zero when industry employment is randomly distributed). These data are provided at the 1987-SIC level; we convert them to 1972-SIC using the Bartelsman, Becker, and Gray concordance.

To provide some description of our measures of industrial immobility, in table A1 we list the highest and lowest values for each measure at the 3-digit SIC (3-digit values are computed by averaging over the values for the 4-digit industries within the 3-digit category). We also include descriptive statistics of our measures in table A2.

Appendix Table A1: High and Low Values of Immobility Variables

Plant Costs			
Highest Values		*Lowest Values*	
324	Cement, hydraulic	274	Miscellaneous publishing
321	Flat glass	273	Books
266	Building, paper and board mills	375	Motorcycles, bicycles and parts
261	Pulp mills	201	Meat products
221	Weaving mills - cotton	272	Periodicals
Agglomeration Economies			
Highest Values		*Lowest Values*	
227	Floor covering mills	302	Rubber and plastic footwear
228	Yarn and thread mills	205	Bakery products
222	Weaving mills and synthetics	271	Newspapers
225	Knitting mills	323	Products of purchased glass
213	Chewing and smoking tobacco	276	Manifold business forms
Transport Costs			
Highest Values		*Lowest Values*	
271	Newspapers	334	Secondary nonferrous metals
324	Cement, hydraulic	372	Aircraft and parts
325	Structural clay products	391	Jewelry, silverware and plated ware
327	Concrete, gypsum and plastic products	376	Guided missiles, space vehicles and parts
241	Logging camps and logging contractors	357	Office and computing machines

Appendix Table A2: Means of Immobility Variables

Transport costs	estimated industry fixed effects : panel regression controlling for distance (authors' construction)	0.009 (0.034)
Plant fixed costs	Bartelsman, Becker, and Gray	0.237 (0.140)
Agglomeration economies	Ellison and Glaeser	0.051 (0.075)

Notes to Table: In the regressions of Table 3, each of these variables is multiplied by the environmental cost ' to construct the interaction terms.

Environmental Costs:

The environmental cost variable is gross annual pollution abatement operating costs as a percentage of total materials costs. Pollution abatement expenses are taken from the *Current Industrial Reports: Pollution Abatement Costs and Expenditures reports by the Census Bureau/U.S. Department of Commerce*, 1972-92. The data from 1989-92 are provided at the 4-digit 1987 SIC level; we used the concordance described in the *NBER Manufacturing Productivity Database* to allocate those data to 1972 SIC industries. Pollution abatement operating costs include all costs of operating and maintaining plant and equipment to abate air or water pollutants, and expenses to private contractors or the government for solid waste management. Pollution abatement operating costs were not collected in 1987, and totals by industry were not reported in 1979, so these years are dropped from our sample. Due to the incompatibility (in the treatment of small plants) between the data collected in the first several years and later years, we include only data since 1978. Materials costs (the denominator) is taken from the *NBER Manufacturing Productivity Database* (Bartelsman, Becker, and Gray).

Net Imports and Tariffs:

The net import variable is the customs value of imports minus exports, scaled by industry shipments. The measure of tariffs is the ratio of duties paid to customs value. Both are taken from the *NBER Trade Database*, available on Robert Feenstra's website. Imports and exports are provided at the level of 4-digit 1972 SIC codes. Value of shipments is taken from Bartelsman, Becker, and Gray.

This database provides data on U.S. customs duties for 1972-94. For 1989-94, these data are provided at the 4-digit 1987 MSIC level. We converted these data to 1972 MSIC industries using the concordance provided in the Feenstra (NBER) CD-rom (which allocates 1987 MSIC imports to 1972 industries in proportion to their 1988 customs value ratios—import data for 1988 are presented for both 1972 and 1987 MSIC industries). Data for all years are then converted from 1972 MSIC to 1972 SIC using a concordance provided by Chris Magee. Dividing by total import volume gives a measure of the average ad valorem tariff.

Human and Physical Capital Shares:

The variable for human capital share is total payroll minus payments to unskilled labor, scaled by industry value added. The measure for physical capital share is payroll's share of value added subtracted from unity. Payments to unskilled labor are estimated as the number of workers in the industry multiplied by the average annual income of workers with less than a high school education in the industry (income data were computed for each year from the *Current Population Survey*, May supplemental surveys). Payroll data and value added are taken from Bartelsman, Becker, and Gray (provided at the 4-digit 1972 SIC level).