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Abstract

Expenditures for environmental protection in the U.S. are estimated to exceed $150 billion annually or about 2% of GDP. This estimate, based on largely self-reported information, is often cited as an assessment of the burden of current regulatory efforts and a standard against which the associated benefits are measured. Little is known, however, about how well reported expenditures relate to true costs. The potential for both incidental savings and uncounted burdens means that actual costs could be either higher or lower than reported expenditures.

A significant literature supports the notion that increases in reported environmental expenditures probably *understate* actual economic costs. Estimates of the true cost of a dollar increase in reported environmental spending range from $1.50 to $12.

This paper explores the relationship between reported expenditures and economic cost in the manufacturing sector in the context of a large plant-level data set at the four-digit SIC level. We use a cost function modeling approach which treats both environmental and non-environmental production activities as distinct, unrelated cost minimization problems for each plant. We then explore the possibility that these activities are, in fact, related by including reported regulatory expenditures in the cost function for non-environmental output. Under the null hypothesis that reported regulatory expenditures accurately measure the cost of regulation, the coefficient on this term should be zero.

In ten of eleven industries studied, including all of the heavily regulated industries, this null hypothesis is accepted using our preferred fixed-effects model. Our best estimate, based on an expenditure weighted average of the four most heavily regulated industries, indicates that an incremental dollar of reported environmental expenditure reduces non-environmental production costs by eighteen cents with a standard error of forty-two cents. This is equivalent to saying that total costs rise by eighty-two cents for every dollar increase in reported environmental expenditures. Using an alternative pooled model we find uniformly higher estimates. Although consistent with previous results, we believe these higher estimates are biased by omitted variables characterizing differences among plants.

Summarizing, our results enable us to reject claims that environmental spending imposes large hidden costs on manufacturing plants. In fact, our best estimate indicates a modest though statistically insignificant *overstatement* of regulatory costs.

Key words: environmental costs, fixed-effects, translog cost model

JEL Classification No(s): C33, D24, Q28

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The Cost of Environmental Protection?

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1 Introduction

Expenditures for environmental protection in the U.S. are estimated to exceed $150 billion annually or about 2% of GDP. This estimate, based on largely self-reported information, is often cited as an assessment of the burden of current regulatory efforts and a standard against which the associated benefits are measured. Little is known, however, about how well reported expenditures relate to true economic costs. Reported expenditures in the manufacturing sector reflect expenses that the plant manager identifies with environmental protection. Yet, the cost to society depends on the resulting changes in total production costs and output prices. Increases in reported environmental expenditures at the plant level may or may not result in dollar-for-dollar increases in production costs. Specifically, the change in production costs depends on whether an increase in reported environmental expenditures incidentally saves money, involves uncounted burdens, or has no other consequence.[2]

Most research on this distinction between reported environmental expenditures and total production costs has focused on the possibility that the former may *understate* the latter. Studies have examined a number of issues, including the possible "crowding out" effect of environmental expenditures on other productive investments, the importance of the so-called "new source bias" in discouraging investment in more efficient facilities, and the potential loss of operational flexibility

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[2]Our focus on production costs as the correct measure of the resource cost associated with environmental protection assumes that no monopolistic rents exist. If firms collect such rents, we would also need to consider the effect of regulation on these rents (e.g., producer surplus) in order to estimate the economic cost of regulation.

associated with environmental controls. It has also been suggested that reported environmental expenditures fail to capture significant managerial and other overhead costs allocable to environmental protection. Data collected by industry, based on broader definitions of environmental expenditure than those used by the Census Bureau, generally yields larger estimates.

In contrast, more limited research suggesting an overstatement of costs has explored the possibility that there is complementarity between pollution control and other production activities.[3] That is, the costs of jointly producing conventional output and a cleaner environment may be lower than if each were produced separately.[4] Generally, only anecdotal information, along with some limited case studies, support the notion that pollution control expenditures may be partially (or wholly) offset by efficiency gains elsewhere in the firm.

Whether a $1 increase in reported environmental expenditures translates into changes in total production costs of more or less than a $1 involves netting out a number of complex, often competing effects. Frequently posed in terms of competitiveness or productivity, the prevailing view in the economics literature is that an incremental $1 of reported environmental expenditures probably increases total production costs by more than $1. Recent studies suggest that total costs may rise by as much as $12 for every $1 of reported expenditures.

In our attempt to address this relationship between reported environmental expenditures and true economic costs, we merge several large data sets containing plant-level information on reported regulatory expenditures as well as prices and quantities of both inputs and outputs. The sample consists of more than 800 different manufacturing plants for multiple industries at the 4-digit SIC level over the period 1979-1991. We employ a cost-function modeling approach that involves three basic steps. First, we distinguish between environmental abatement expenditures and non-environmental production expenditures. Second, we model both the production of conventional output and environmental services as distinct cost minimization problems and derive

[3]There is also a literature comparing *ex ante* to *ex poste* costs. That is a related but distinct issue from the one addressed in this paper.

[4]Suppose a potential investment both increases efficiency and reduces pollution but has a slightly lower rate of return than a firm's other investment opportunities. When environmental regulation is imposed, the cost of compliance might be relatively small since the necessary investment was almost profitable when the environmental benefits were ignored.

expressions for the resulting cost and factor input shares. Third, we estimate our cost model *inserting* a term that reflects the possible impact of environmental expenditures on non-environmental production. Under the null hypothesis that reported environmental expenditures accurately reflect costs, the coefficient on this term should be zero. Our approach differs from previous work with similar data by using a cost-function modeling approach that distinguishes the production of environmental services and conventional output, by considering a larger number of industries, and by paying particular attention to plant-specific effects.

Our main results, based on four large, heavily regulated industries, indicate large but statistically insignificant variability across industrial sectors. We find that reported environmental expenditures tend to generate offsetting savings in some industries and added burdens in others. However, none of the estimated offsets is statistically significant from zero. Using our preferred fixed-effects model, a weighted average of the four industries yields a best estimate of aggregate savings in conventional production costs of eighteen cents for every dollar of reported incremental pollution control expenditures, with a standard error of forty-two cents. This is equivalent to saying that total production costs rise by eighty-two cents (one dollar minus eighteen cents) for every dollar of reported environmental expenditures. Estimates based on an alternative, pooled model consistently show smaller savings and larger additional burdens than the fixed-effects model. Although the higher, pooled estimates are more consistent with previous work, we believe they are biased by omitted variables characterizing differences among plants.

In the remainder of the paper, we first review the literature surrounding cost estimates of environmental protection. We then present an industry level model of plant behavior in the presence of environmental regulation using a cost function approach. In Section 4 we discuss the estimation of this cost function and use the results to compute the marginal cost associated with reported environmental expenditures. Section 5 offers a set of concluding observations. Details concerning construction of the dataset and estimation of the model parameters are contained in the Appendix.

2 Background

2.1 Distinguishing Reported Expenditures and Economic Costs

The key issue addressed in this paper is the possible gap between the true cost of environmental regulation and readily available, self-reported expenditure estimates. To obtain an accurate measure of true economic cost, one can imagine plant managers providing accurate responses to the following (hypothetical) question:

> "Identify the increase in costs associated with your efforts to reduce environmental emissions or discharges from your facility. In preparing your estimates, be sure to consider the extent to which environmental activities : (a) involve direct outlays of capital and operating costs; (b) reduce other (i.e., non-environmental) capital and operating costs; (c) lead to cost-saving innovations; (d) affect operating flexibility; (e) crowd out non-environmental investments; or (f) discourage purchase of new equipment because of differential performance requirements for new versus existing equipment. Include estimates of the plant managers' time and other overhead items associated with these activities. Exclude expenditures related to occupational health and safety. When process changes (as opposed to end-of-the pipe additions) are involved, allocate only that portion of the costs attributable to environmental protection."

Of course few firms possess the information to reliably answer such a complex and comprehensive question. Instead, we have at our disposal the Pollution Abatement Costs and Expenditures (PACE) Survey. Collected by the U.S. Census Bureau in most years 1973-1994, the PACE questionnaire asks a sample of manufacturing plants to provide information on capital and operating expenditures, including depreciation, labor, materials, energy and other inputs – essentially item (a) of our hypothetical question.[5]

PACE results have been regularly published by Census and represent, by far, the most comprehensive source of information on environmental expenditures. They form the basis of calculations that annualized environmental costs exceed $150 billion (U.S. Environmental Protection Agency

[5] The Census Bureau ceased collecting PACE in 1994 for budgetary reasons. The PACE questionnaire asks plant managers how expenditures compare to what they would have been in the absence of environmental regulation. This raises the issue of the appropriate baseline. Absent regulation, firms might still engage in some pollution control to limit tort liability, maintain good relations with communities in which they are located, maintain a good environmental image, and other reasons. However, it is unclear whether survey respondents are able to determine what environmental expenditures would have been made in the absence of regulation.

1990).[6] PACE data have been used as inputs in dynamic general equilibrium analyses to estimate the long-run consequences of environmental regulation. These results indicate social costs which are from 30 to 50 percent higher than reported annual expenditures (Hazilla and Kopp 1990; Jorgenson and Wilcoxen 1990). PACE data have also been used to analyze the decline in productivity growth observed during the 1970's. For example, researchers have found that environmental regulations accounted for 8 to 44 percent of the declines in total factor productivity observed in various industries (U.S. Office of Technology Assessment 1994).

Despite the broad use of PACE data and the widespread presumption that it measures economic costs, numerous issues distinguish PACE data from true economic costs. When firms report operating expenses, for example, it is unclear how they handle management time and other overhead items. As discussed by Noreen and Soderstrom (1994), treating overhead as either completely variable or completely fixed is generally wrong – leading to over- or understatement of costs, respectively. Various studies also suggest that responses to items (d-f) in the hypothetical question would likely raise estimates of costs above those implied by the expenditure data alone (for excellent surveys, see Jaffee, Peterson, Portney, and Stavins 1995; Schmalensee 1993). There is some evidence, for example, that environmental investments may crowd out other investments by firms (Rose 1983). Further, many environmental regulations mandate stringent standards for new plants but effectively exempt older ones from requirements. This new source bias may discourage investment in new, more efficient facilities and thereby raise production costs (Gruenspecht 1982; Nelson, Tietenberg, and Donihue 1993). It has also been suggested that pollution control requirements may reduce operating flexibility which, in turn, could also lead to higher costs (Joshi et al. 1997). Industry estimates of pollution control expenditures, using broader definitions of cost than those used by the Census Bureau, routinely generate higher estimates. One recent industry estimate was almost double the PACE number (American Petroleum Institute 1996).

[6] The EPA estimates are somewhat higher than those developed by Census Bureau largely because: 1) EPA annualizes investment outlays (at a 7 percent discount rate) rather than directly reporting annual expenditures; and 2) the EPA data includes some programs not covered by Census, e.g., drinking water and Superfund.

2.2 The Potential for Overstatement

In contrast to items (d-f), (b) and (c) in the hypothetical survey question represent a very different line of thinking. Item (b) addresses the argument that potential complementarities between conventional production and environmental expenditures may offset part of the reported environmental expenditures. Especially when process changes are involved (as opposed to end-of-the-pipe treatment), the cost of jointly producing both conventional output and a cleaner environment may be lower than the cost of producing them separately. Such complentarities might arise, for example, from cost savings associated with recovered or recycled effluents. The PACE survey has attempted to estimate these so-called offsets but they are among the items thought to be most subject to measurement error (Streitweiser 1996).

Another complementarity story arises when the costs of shutting down a production line are substantial. Once it becomes necessary to stop production in order to make environmentally motivated modifications, it is only natural that other, non-environmentally motivated projects might also be undertaken. This "harvesting" of non-environmental projects alongside necessary environmental ones reduces the expenses associated the non-environmental projects, leading to a complementarity.

Item (c) represents the notion that environmental requirements may stimulate plant managers to innovate and thus may offset some of the added costs associated with environmental protection. The underlying argument has its roots in the work of Leibenstein (1966) and others who have written about suboptimal firm behavior. The application to environmental issues goes back at least to Ashford, Ayers, and Stone (1985). The most recent discussion is associated with Porter (1991) who claims that "environmental standards can trigger innovation that may partially or more than fully offset the costs of complying with them" (Porter and Van der Linde 1995). In effect, the argument is that the complementarities between environmental activities and conventional production (item b) combined with the induced innovations associated with environmental requirements (item c) may actually exceed the direct expenditures associated with environmental protection (item a).

The empirical basis for assessing these claims is quite limited. A study by Meyer (1993), which examines whether states with strict environmental laws demonstrate poor economic performance relative to states with more lax standards, is frequently cited in support of the Porter hypothesis. Althought the study found that states with stricter laws actually performed better, the paper sheds little light on a possible causal relationship between regulation and economic performance because it does not control for many of the factors relevant to a state's economic performance. Various case studies of particular plants have been conducted but problems of selection bias make it impossible to generalize from the results (Palmer, Oates, and Portney 1995).

Most economists have been unsympathetic to Porter's arguments because they depend on the assumption that firms consistently ignore or are ignorant of profitable opportunities, including the use of innovative technologies (Palmer and Simpson 1993). This skepticism does not preclude specific instances where government regulations may lead to cost savings, e.g., the well-known case of controls on vinyl chloride emissions (Doniger 1978). Alternatively, others have conjectured that environmental regulation could have the effect of lowering costs – at least at the industry level – by forcing exceptionally inefficient plants to close and thereby expanding production at the remaining, more efficient facilities (U.S. Office of Technology Assessment 1980). Still, these examples are generally regarded as special cases and considered atypical of behavior in a competitive economy.

2.3 Empirical Studies of PACE Data

Actual plant-level responses to our hypothetical survey question would enable researchers to measure the relative importance of the various, often countervailing influences. Absent such detailed, data, we can only estimate the net effect based on available PACE and Census information. Several other papers have also attempted to do this. Work by Gray (1987) and Gray and Shadbegian (1994) explored these issues in the context of growth accounting. Using a straightforward model where environmental activities are entirely separate from conventional production, they show that a 1% increase in the ratio of environmental expenditures to total costs should lead to a 1% fall in measured total factor productivity. Any deviation from this one-for-one relation indicates joint production; in their terminology, productivity effects. Their results indicate a more than one-for-one fall in measured productivity, suggesting that the cost of regulation is understated by reported environmental expenditures. In the steel industry, for example, they find a $3.28 increase in total costs for every additional dollar of environmental expenditure.

Similar work by Joshi, Lave, Shih, and McMichael (1997) (hereafter, JLSM) focuses on the steel industry over the period 1979-88. JLSM distinguish between the *direct* effects of regulation (i.e., the reported abatement expenditures) and the *indirect* effects reflecting any difference between reported expenditures and changes in total production cost.[7] JLSM estimate a cost function in which pollution abatement expenditures enter as a fixed output, finding that the indirect effects of regulation are large – on the order of $7-12 for each $1 in reported expenditures.

Our approach differs from previous work with similar data by considering a larger number of industries, using a cost-function modeling approach that distinguishes the production of environmental services and conventional output, and paying particular attention to plant-specific effects. We prefer this method to the growth accounting framework of Gray and Shadbegian because it more closely resembles the plant-level decision problem. Namely, prices are fixed and the plant seeks to minimize costs, making costs and factor inputs the endogenous quantities. The growth accounting framework, in contrast, treats factor inputs as fixed and output as the endogenous variables.[8]

In contrast to JSLM, we adopt a cost function approach that allows for the possibility of disjoint environmental and non-environmental activities under our null hypothesis. Their joint modeling approach, while flexible, implicity rules out the possibility that environmental activities are unrelated to conventional production activities. By extension, this also eliminates the possibility that reported environmental expenditures exactly measure true economic costs.[9]

We distinguish ourselves from both the Gray and Shadbegian and JSLM studies, however, in

[7] Gray (1987) refers to these indirect effects as the *real* effect of regulation.

[8] This assumes that productivity shocks are uncorrelated with factor inputs and that the scale of regulatory expenditures depends on the level of inputs rather than the level of output.

[9] By modeling the log of total costs as a linear function of the log of reported regulatory expenditure, it is impossible for the derivative of total cost with respect to regulatory expenditure to identically equal one in the JSLM framework. The derivative in levels depends on the ratio of total costs divided by regulatory expense which varies across observations. We instead specify our relation in terms of the log of *non-environmental* production costs so that a zero coefficient on regulatory expense reflects complete separation of environmental and non-environmental production.

our treatment of differences among plants. While we are able to replicate their general results in Section 4, we show that those results depend critically on strong assumptions about homogeneity among plants.[10] Specifically, they assume that differences in plant location, age and management have no effect on either productivity or environmental expenditure – an assumption that seems unlikely to be satisfied in practice. Allowing for such differences (by estimating a fixed-effects rather than a pooled model) substantially reduces the estimated economic cost associated with an incremental dollar of reported expenditures. Our results, in fact, allow us to statistically reject the hypothesis that the economic cost of an additional dollar of reported environmental expenditure is much more than one dollar.

3 Model

The most transparent way to measure the relation between changes in total costs and changes in reported PACE expenditures would be to focus on two identical groups of plants, one of which is randomly subject to higher regulatory standards. Using this data we could simply examine the difference in average non-environmental production costs between the two populations and then compare it to the difference in average reported PACE expenditures. The ratio of the differences would reveal the degree, if any, to which the reported PACE data over- or understates true costs. Since the two groups would be otherwise identical (due to randomization), this would yield an unbiased estimate of any potential savings or uncounted burdens.

In the absence of such a transparent, randomized experiment, we are forced to construct a more complete model of production. This model must adequately account for other factors besides regulation which affect costs. If we fail to do this, the influence of these factors may be falsely attributed to regulation.

An important source of such confounding influence may be unobservable productivity differences among plants. These differences, which might be related to geographical location, man-

[10]Gray and Shadbegian report results allowing for plant heterogeneity but argue that they are more likely to be affected by measurement error. As we discuss in Section B.2, this is not necessarily the case and, even if it were, there are other compelling reasons to prefer the results allowing for heterogeneity.

agement style, age, or other plant characteristics, could influence the level of both environmental and non-environmental expenditures. Simple pooling of the data to estimate the cost implications of higher reported PACE expenditures without controlling for these differences would be equivalent to asking what happens when regulatory expenditures change *along with* associated changes plant location, management style and age. To the extent that we are interested in the economic cost of higher environmental expenditures holding plant characteristics constant, this constitutes an omitted-variable bias.

Since our data set contains multiple observations for each plant, we have the ability to consider fixed-effects models which explicitly accomodate plant-level differences in productivity. The downside to this approach is that between-plant variation in costs will be ascribed to these fixed effects. Thus, the uncounted effects of more expensive regulation are estimated solely by examining changes in non-environmental expenditures *over time* associated with changes in reported PACE expenditures *over time*. If there are, in fact, no productivity differences among plants, this approach leads to unnecessarily noisier and less efficient parameter estimates. This potential loss of efficiency is the cost of protecting ourselves against omitted-variable bias.

3.1 General Approach

Our analytic approach involves three distinct steps. First, we distinguish between environmental abatement expenditures and non-environmental production expenditures. This distinction allows us to consider the null hypothesis that conventional, non-environmental production expenditures are unaffected by PACE activities. Second, we model the production of both environmental services and conventional output as distinct cost minimization problems and derive expressions for the resulting cost and factor input shares. Third, we estimate our cost model *inserting* a term that reflects the possible impact of environmental expenditures on non-environmental production. If regulatory efforts and conventional production are, in fact, distinct, this term will not be statistically significant and we would conclude that reported environmental expenditures are a good assessment of the true costs. Conversely, if the inserted environmental expenditure variable is significant, then one would conclude that reported environmental expenditures are *not* an accurate measure of the cost of environmental regulation.

Estimation of our cost model is complicated by two data limitations: We cannot separate observed factor inputs into those used for abatement efforts and those used for conventional productions. Also, we do not observe a "level" of environmental output. These limitations lead us to make stronger identifying assumptions but do not fundamentally hinder our approach.

The first step, separating total expenditures into abatement effort and conventional production, allows us to focus squarely on the hypothesis of interest. Under the null hypothesis that reported environmental expenditures accurately reflect the cost of environmental regulation, the remaining expenditures on conventional production should be completely determined by the level of conventional output, prices of inputs, a time trend, and possible idiosyncratic differences among plants.

This leads to the second step, where we derive distinct expressions for expenditures on both conventional production and abatement based on cost minimizing behavior by plants. By adopting a structural approach, with model parameters representing technological constraints rather than simple correlations in the data, we have greater confidence that the parameters are invariant to changes in regulatory policy. Our cost function approach also assumes output and prices are exogenous. The plant then chooses the cost-minimizing combination of endogenous inputs. This, in turn, determines expenditures on abatement and conventional output.

Our cost function approach has two advantages over conventional production function approaches, which instead treats inputs as exogenous and outputs as endogenous. First, we avoid regressing output on regulatory expenditure which may be biased if the scale of output influences regulatory costs.[11] Second, we take advantage of the first order conditions for cost minimization to impose additional restrictions on our parameters and improve estimation efficiency.

The third step of our analysis allows for potential economies of scope.[12] Specifically, we insert regulatory expenditures into our model of conventional, non-environmental production costs.

[11] Gray and Shadbegian (1994), for example, find that scaling abatement expenditures by output tends to bias their cost estimates upward.

[12] See Bailey and Friedlaender (1982).

This gives us an idea, loosely speaking, of the possible complementarities involved in the joint production of conventional output and environmental activities. To the extent that environmental activities are completely disjoint from regular production, the coefficient on regulatory expenditures will be zero. In that case, the marginal economic cost of an additional dollar of environmental expenditures will be entirely reflected by environmental expenditures alone and will exactly equal one dollar.

If, however, environmental expenditures somehow complement conventional production, the marginal cost could be less than a dollar. As noted, such complementarities might arise from process changes which were almost profitable even without environmental considerations, from benefits associated with recovered and recycled effluents, or from unforeseen spillovers caused by regulatory activities. Alternatively, such efforts might impose additional, uncounted costs. The possible crowding out of productive investments, higher administrative costs, and loss of flexibility, for example, are presumably not counted in the measure of reported regulatory expenditures and could lead to a marginal economic cost exceeding one dollar.

3.2 Specification

We now explain the key technical aspects of our model. In each period t we assume each plant i wishes to minimize the cost associated with producing a given quantity of conventional output Y. The function $F_{i,t}(\cdot)$ defines a production technology involving Y coupled with inputs of capital, labor, energy, and materials. In particular, $F_{i,t}(\cdot) = 0$ is the production *frontier* and $F_{i,t}(\cdot) < 0$ describes feasible but inefficient input/output combinations.[13] The production function is indexed over both plants i and time t to allow for exogenous time trends in productivity as well as differences between plants.

[13] That is, given any feasible production combination it is always possible to use more inputs or produce fewer outputs by simply discarding the excess. However, unless some prices are zero this will not be efficient.

The cost minimization performed by the plant is given by:

$$PC = \min_{K,L,E,M} P_k K + P_l L + P_e E + P_m M \quad \text{such that } F_{i,t}(Y,K,L,E,M) \leq 0 \text{ with } Y \text{ fixed,} \tag{1}$$

where P_k, P_l, P_e, and P_m represent prices of capital, labor, energy and materials, respectively, and PC is the production cost associated with Y.

The minimization in (1) defines a cost function $PC = G_{i,t}(Y, P_k, P_l, P_e, P_m)$.[14] We specify the cost function to be of the translog functional form:[15]

$$\log(PC) = \alpha_i + \alpha_{i,x}' \cdot X + \frac{1}{2} X' \beta_x X + \alpha_r \log R \tag{2}$$

where $X = \{\log Y, \log P_k, \log P_l, \log P_e, \log P_m, t\}'$, $\alpha_{i,x} = \{\alpha_y, \alpha_{i,k}, \alpha_{i,l}, \alpha_{i,e}, \alpha_{i,m}, \alpha_t\}'$, $\beta_x = \begin{bmatrix} \beta_y & \beta_k & \beta_l & \beta_e & \beta_m & \beta_t \end{bmatrix}$, $\beta_y = \{\beta_{yy}, \beta_{yk}, \beta_{yl}, \beta_{ye}, \beta_{ym}, \beta_{yt}\}'$, etc., and R is regulatory expenditure. Note that we have assumed that plant differences as well as a time trend may change overall productivity and bias the factor shares (see factor share equations below). We have also added a term ($\alpha_r \log R$) reflecting the possible influence of regulatory expenditures on conventional production costs. Our null hypothesis is that the coefficient α_r should be zero and that reported regulatory expenditures accurately reflect the cost of environmental regulation. That is, PACE activities should have no effect on non-environmental production costs.

Taking the first derivatives of this log cost function with respect to log prices yields expressions for the associated factor shares by Shepard's Lemma:

$$v_{k,y} = \alpha_{i,k} + \beta_k' X$$
$$v_{l,y} = \alpha_{i,l} + \beta_l' X$$
$$v_{e,y} = \alpha_{i,e} + \beta_e' X$$
$$v_{m,y} = \alpha_{i,m} + \beta_m' X$$

where $v_{k,y}$, $v_{l,y}$, $v_{e,y}$ and $v_{m,y}$ are the input cost shares for capital, labor, energy and materials,

[14] For a general discussion of cost functions see Varian (1992).

[15] See Diewert and Wales (1987) for a discussion of the translog and other flexible functional forms.

respectively, associated with producing Y units of output, and $\beta_k = \{\beta_{ky}, \beta_{kk}, \beta_{kl}, \beta_{ke}, \beta_{km}, \beta_{kt}\}'$, etc. As noted above, our specification allows for plant specific differences in factor demand.[16]

Normally, we would proceed with the simultaneous estimation of both the cost function (2) and the share equations. However, the cost shares associated with Y production costs ($v_{k,y}$, $v_{l,y}$, etc.) are not, in fact, observed. Instead, we observe the cost shares associated with both production costs *and* abatement costs. These "aggregate" cost shares are a weighted average of production and abatement cost shares. The aggregate cost shares will not equal the production cost shares except under the very restrictive assumption that the technology for producting output Y and abatement are almost identical.

To work around this difficulty, we specify cost share relations for regulatory costs:

$$v_{k,r} = \gamma_k + \delta_k' Z$$

$$v_{l,r} = \gamma_l + \delta_l' Z$$

$$v_{e,r} = \gamma_e + \delta_e' Z$$

$$v_{m,r} = \gamma_m + \delta_m' Z$$

where $v_{k,r}$, $v_{l,r}$, $v_{e,r}$ and $v_{m,r}$ are the input cost shares associated with regulatory costs for capital, labor, energy and materials, respectively, $Z = \{\log P_k, \log P_l, \log P_e, \log P_m, t\}'$, $\{\gamma_k, \gamma_l, \gamma_e, \gamma_m\}$ are constants, and $\delta_k = \{\delta_{kk}, \delta_{kl}, \delta_{ke}, \delta_{km}, \delta_{kt}\}'$, etc. Because we are unable to observe an output measure associated with regulatory efforts (analogous to Y) we cannot estimate a corresponding cost function for regulatory costs.[17] We are also forced to ignore potential factor biases associated with the scale of regulatory activity.

[16]The number of fixed effects being estimated raises the issue of whether the remaining model parameters can be consistently estimated – an incidental parameter problem (Neyman and Scott 1948). Chamberlain (1980) has shown that maximizing the likelihood conditional on statistics that are sufficient for the incidental parameters avoids this problem. Cornwell and Schmidt (1992) show that the conditional and unconditional MLEs coincide for a system of equations with fixed effects, proving the consistency of MLE in this case. Our model deviates slightly from their model because we include fixed effects interacted with input prices in the cost function. This does not, however, affect the consistency of the remaining parameter estimates.

[17]Without a measure of regulatory effort we have no way to explain the scale of regulatory costs.

The aggregate factor shares,

$$
\begin{aligned}
v_k =& \frac{R}{PC+R} v_{k,r} + \frac{PC}{PC+R} v_{k,y} \\
v_l =& \frac{R}{PC+R} v_{l,r} + \frac{PC}{PC+R} v_{l,y} \\
v_e =& \frac{R}{PC+R} v_{e,r} + \frac{PC}{PC+R} v_{e,y} \\
v_m =& \frac{R}{PC+R} v_{m,r} + \frac{PC}{PC+R} v_{m,y}
\end{aligned}
$$

where R are regulatory costs and PC are production costs, can now be estimated alongside the production cost function. To be consistent with economic theory, we impose symmetry ($\beta_{ij} = \beta_{ji}, \delta_{ij} = \delta_{ji}$) and homogeneity of degree one on prices. That is, a doubling of prices doubles total costs.

One of these four share equations is redundant by the price homogeneity restrictions and is therefore dropped from the estimation procedure.[18] We add a vector of normal, independent and identically distributed stochastic disturbances to the cost function plus three share equations and estimate the following system (now explicitly indexed over plants and time): [19]

$$
\begin{aligned}
\log(PC_{i,t}) &= \alpha_i + \alpha_{i,x} \cdot X_{i,t} + \frac{1}{2} X'_{i,t} \beta_x X_{i,t} + \alpha_r \log R_{i,t} + \epsilon_{1,i,t} \\
v_{k,i,t} &= \frac{R_{i,t}}{R_{i,t} + PC_{i,t}} \left(\gamma_k + \delta'_k Z_{i,t}\right) \frac{PC_{i,t}}{R_{i,t} + PC_{i,t}} \left(\alpha_{i,k} + \beta'_k X_{i,t}\right) + \epsilon_{2,i,t} \\
v_{l,i,t} &= \frac{R_{i,t}}{R_{i,t} + PC_{i,t}} \left(\gamma_l + \delta'_l Z_{i,t}\right) \frac{PC_{i,t}}{R_{i,t} + PC_{i,t}} \left(\alpha_{i,l} + \beta'_l X_{i,t}\right) + \epsilon_{3,i,t} \\
v_{e,i,t} &= \frac{R_{i,t}}{R_{i,t} + PC_{i,t}} \left(\gamma_e + \delta'_e Z_{i,t}\right) \frac{PC_{i,t}}{R_{i,t} + PC_{i,t}} \left(\alpha_{i,e} + \beta'_e X_{i,t}\right) + \epsilon_{4,i,t}
\end{aligned}
\tag{3}
$$

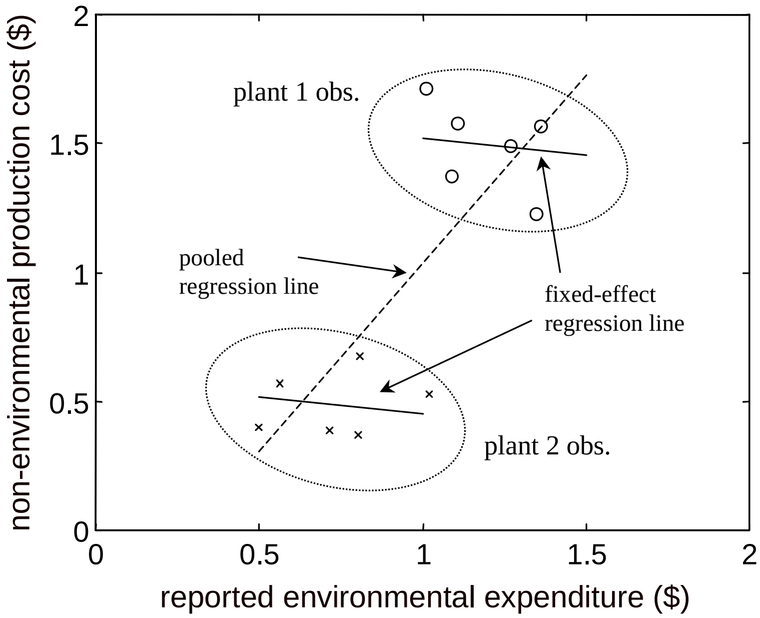
3.3 Accounting for Plant-Level Differences

The model given in (3) is an extremely flexible specification of plant-level technology. By allowing the parameters α_i and $\alpha_{i,x}$ to vary among plants (indexed by i), it is possible that two plants

[18] We omit the share equation for materials and express all nominal quantities as ratios with respect to the price of materials. However, the estimated parameters are invariant to the choice of which equation is omitted. For a complete discussion of translog cost function estimation, see discussion in Berndt (1990), Chapter 9.4.

[19] We allow for contemporaneous correlation of the disturbance, e.g., between $\epsilon_{1,i,t}$ and $\epsilon_{2,i,t}$. Note, however, that the disturbance in the unspecified R cost function must be uncorrelated with ϵ in order for the parameter estimates to be unbiased (since R occurs in the production cost function). This is reasonable if we view the environmental cost minimization as occuring *before* the non-environmental cost minimization.

Figure 1: Fixed Effects versus Pooled Estimator[a]



[a]This is a stylized representation of the model given in Equation (2). It emphasizes the role of fixed effects but ignores other covariates besides regulation as well as the log-log specification.

producing the same amount of output and facing the same input prices may have different production costs and may use different combinations of inputs. Requiring some of these parameters to be similar across plants then allows us to explore more restrictive models.

We could assume, for example, that there is no variation in any of the α's across plants. That would be the case if all plants shared exactly the same production technology. Under this assumption, we could estimate the model by simply pooling the data and ignoring the panel structure (i.e., multiple observations for each plant).

To estimate the full model, we take advantage of the panel structure of the data, allow the α's to differ and estimate them along with the other parameters. We do this by adding dummy variables for each plant in both the cost function and share equations and otherwise following the same procedure as before.[20]

Figure 1 illustrates the potential discrepancy between these assumptions. Consider data from two plants with six observations of production costs and regulatory expenditures for each. As

[20]The dummy variables appearing the share equations also appear in the cost function interacted with the corresponding prices.

drawn, there is a negligible effect of rising regulation on production costs for both plant 1 (denoted by $\circ$) and plant 2 (denoted by $\times$) *viewed separately*. If we view each plant separately – but require increased environmental expenditures to have the same incremental effect – it would appear that there is a roughly \$0.20 decrease in production costs for every \$1 increase in regulation. That is, costs are *overstated* by reported expenditures. This is the fixed-effects estimate.

However, plant 1 has, on average, \$0.50 more regulation than plant 2 and, on average, \$1 more production costs – an increase of \$2 in production costs per dollar of regulation. Pooling the data, in effect averaging the zero-for-one fixed-effects relation with this two-for-one relation, we estimate a pooled slope coefficient of 1.5. That is, based on the pooled estimate, costs are substantially *understated* by reported expenditures (by nearly 150%).

If there was no discrepancy between these two relations – that is, the relation among plant means versus the relation among observations for each plant – then the fixed-effects and pooled slope estimates would be roughly the same. Visually, the data points in Figure 1 would lie along the same (dotted) line, rather than along two different (solid) lines. In this scenario, the pooled estimator would be preferred since it uses more information (the relation between plant means) than the fixed-effects estimator. This is especially important if there is more variation between plants than within plants, as is usually the case.[21]

When there is a discrepancy, as illustrated in Figure 1 and as we find in our data, it is not immediately obvious which of the two slope estimates – the fixed-effects or the pooled – is preferred.[22] If we believe that the differences between plants are actually *caused* by differences in regulation, then the pooled estimator is appropriate. Suppose, for example, that both regulatory expenditures and total costs differ by plant location. In such a scenario, firms might only be willing to select locations with higher regulatory expenditures if the other costs at that location were lower, at least partially offsetting the higher regulatory costs. In that case, we might be interested in a slope coefficient which included the indirect effect of higher regulation on total cost via the choice of

[21] See Table 2 in Gray and Shadbegian (1994).

[22] We statistically reject the hypothesis that the intercepts are the same in every industry and at any reasonable level of significance. See discussion in Appendix B.

plant location. This would correspond to the pooled estimate, where variation between plants, e.g., location, is used to identify the effect of regulation. The fixed-effect estimate, in contrast, ignores this variation by controlling for all fixed (i.e., time invariant) differences among plants. In light of this distinction, we might view the pooled estimate, which allows plant characteristics to change, as a *long-run* elasticity and the fixed-effects estimate, which holds constant differences between plants, as a *short-run* elasticity.[23]

There are three reasons why this scenario where regulation causes plant differences is inappropriate and why we instead prefer the fixed-effects model. First, it seems that there are many more plant characteristics which are likely to influence regulatory costs rather than be influenced by it. If firms choose their plant locations without regard for regulatory costs, even though regulatory differences exist, it would be incorrect to compute an estimator which insinuated that regulation affects location, rather than the other way around.[24] Considering characteristics like age and management style, it becomes even more apparent that plant differences in regulatory expenditure are more likely to be an effect than a cause of other plant differences. If environmental expenditures are, in fact, affected by factors like plant age, location and management, the pooled estimator will suffer from omitted-variable bias while the fixed-effects estimator will remain unbiased.

The second reason for preferring the fixed-effects model is an empirical one. As discussed in Section 4, the pooled estimates are uniformly *larger* than the fixed-effects estimates (as depicted in Figure 1). If the purpose of the pooled estimator is to capture the increased flexibility over the long run, the pooled slope estimate should instead be *smaller*. Thus, there are empirical reasons to reject the pooled results.

Finally, even if we decide to estimate a regulatory effect which includes effects transmitted via differences in plant characteristics, it would be inappropriate to simply pool the data as described.[25]

[23] Caves, Christensen, Tretheway, and Windle (1985) use this distinction to differentiate returns to scale from returns to density in the U.S. railroad industry. Assuming the track network used by firms is fixed over time, they use a fixed-effects model to estimate return to density, holding network fixed, and a random-effects (e.g., pooled) model to estimate return to scale, allowing network size to vary.

[24] Bartik (1988), Bartik (1989), Friedman, Gerlowski, and Silberman (1992), Levinson (1992) and McConnell and Schwab (1990) all find small or insignificant effects of regulation on plant location.

[25] That is because differences in regulatory expenditures are unlikely to explain *all* the differences among plants, leaving a random, unexplained difference in cost which is common among the observations of a given plant. The pa-

Instead, a random-effects model would be appropriate (Mundlak 1978). The random-effects estimator, however, continues to suffer from the first two criticisms, i.e., omitted variable bias and empirical incongruity with theory, again recommending the fixed-effects approach.[26]

4 Measuring the Cost of Environmental Expenditures

To determine the relationship between environmental expenditures and actual cost we estimate the cost function and share equations derived in the previous section. The measure that concerns us – the potential influence of environmental expenditures on non-environmental production costs – is then determined from the estimated parameters.

In particular, the parameter α_r measures the elasticity of non-environmental production costs with respect to reported environmental expenditures. Multiplying this estimated elasticity by the ratio of non-environmental costs to environmental expenditures for a particular plant reveals the dollar change in non-environmental costs for a dollar change in reported environmental expenditures. We refer to this quantity as the non-environmental cost *offset*. Adding one to this number reveals the dollar change in total costs (environmental $+$ non-environmental) for a dollar change in reported environmental costs. We refer to this quantity as the *marginal cost* of reported environmental expenditures.

The cost function and share equations given in (3) are estimated using maximum likelihood. The resulting parameter estimates are reported in Table B.2 and described in Appendix B. It is interesting to note that while just over half of the estimated parameters are significantly different from zero, α_r is significant in only one of the eleven industries.[27] This immediately suggests that there will be little evidence supporting the hypotheses of either understatement or overstatement of regulatory costs.

rameter α_i in this case would be a randomly distributed variable. While a simple pooled estimator would be consistent and unbiased, it would not be efficient nor would the standard errors be correct.

[26] There are potentially two opposing econometric reasons one might prefer the random-effects model over the fixed-effect model – measurement error and endogeneity. This is discussed in Appendix B.2.

[27] The results in the one significant industry, motor vehicles, are questionable based on the unrealistically large estimate of $25 in additional, uncounted costs for every dollar of reported costs.

These results also indicate that our flexible modeling approach captures significant features of the data. Tests that the fixed effects in each equation are zero, for example, are strongly rejected by likelihood ratio tests.[28] Therefore, alternative approaches which assume a simpler relation between regulation and total costs may be misspecified.

4.1 Marginal Regulatory Cost

We examine the connection between environmental expenditures and total costs in terms of marginal changes around the observed level of expenditures. In other words, we estimate the associated change in total costs if current reported environmental expenditures rise by one dollar. An alternative and different question is what fraction of *existing* reported expenditures actually reflect *existing* economic costs – an average cost measure. Unfortunately, this is a more complex question because it requires us to determine the relationship between reported expenditures and economic costs, not only over the range of expenditures which we observe in the data, but all the way back to zero expenditures. Such an extrapolation would not be credible. We do believe, however, that a marginal cost considerably higher or lower than one would suggest a similar directional effect for the average cost relation.

To calculate marginal cost, we differentiate the cost function in Equation (2) with respect to regulatory expenditures R. This yields an estimate of non-environmental offset, O, associated with an increase in reported environmental expenditure,

$$\frac{\partial PC}{\partial R} = O = \frac{PC}{R}\alpha_r \tag{4}$$

where PC is non-environmental production cost, R is regulatory expenditure, and α_r is a parameter estimate from the cost function in Equation (2). Intuitively, this measure reveals the degree to which additional environmental expenditures affect non-environmental expenditures. If this derivative is near zero, increases in reported expenditures are, in fact, a good measure of the additional economic burden of further regulation. If the derivative is not equal to zero, such expenditures

[28] In each test, we compare the difference in the maximized log-likelihood between the pooled and fixed effects models, multiplied by two, to a chi-squared distribution with $4(n-1)$ degrees of freedom, where n is the number of firms in the sample and $4(n-1)$ is the number of additional restriction imposed by the pooled model.

misrepresent incremental costs, with offset values greater than zero indicating an understatement of true costs and offset values less than zero an overstatement.

Using O, we can also compute marginal costs $MC = \partial(PC + R)/\partial R = 1 + O$. That is, the change in *total* costs associated with a change in reported regulatory expenditure. This measure is useful because it summarizes the true cost associated with an incremental dollar of reported environmental expense.

Since the value of O computed in (4) depends on observation specific values of PC and R, these offset measures will vary from observation to observation. In order to compute an aggregate answer, summarizing the offset at the industry or sectoral level, we have to make assumptions about how to weight these different values. Conceptually, we are deciding how an increase in aggregate regulatory expenditures would likely be allocated among firms in the sample. We could compute a simple arithmetic average over all the observations. However, this would amount to dividing up additional expenditures evenly among all observations – even though some plants currently have much lower regulatory expenditures than others. A more plausible alternative would be to consider the aggregate offset of raising environmental expenditures across plants *in proportion* to each plant's current expenditures.[29] That is, plants with small expenditures would have small increases and plants with large expenditures would have large increases. Such a calculation involves a weighted average where the weights correspond to the level of each observation's regulatory expenditure:[30]

$$O_{agg} = \sum_{i,t} \left(\frac{R_{i,t}}{\sum_{j,s} R_{j,s}} \right) \cdot O_{i,t} \tag{5}$$

Aggregate marginal cost then equals one plus the computed value of O_{agg}.

Table 1: Offset Estimates – Large Expenditure Industries
(standard errors are in parentheses)

	Industry:	pulp and paper	plastics	petroleum	steel	cross-industry average
	Full sample					
	# of obs.	615	404	717	536	
①	fixed effect	–0.36	–0.80	–0.22	0.41	–0.18
		(0.26)	(0.56)	(0.76)	(0.42)	(0.42)
②	pooled	–0.03	–0.33	2.47*	2.28*	1.73*
		(0.23)	(0.49)	(0.62)	(0.33)	(0.34)
	weight†	0.130	0.089	0.430	0.166	0.816

†Ratio of expenditures in each industry to eleven-industry total. This weight is used to compute cross-industry averages. See footnote 31 in the text concerning calculation of the weights.

*Significant at the 5% level.

4.2 Industry and Manufacturing Sector Estimates

Tables 1 and 2 present estimates based on equation (5) of the offsetting effects of reported environmental expenditures on nonenvironmental production costs, hereinafter referred to as nonenvironmental offsets. Multi-industry aggregates are shown in the last column(s). Table 1 focuses on four industries with the largest share of regulatory expenditures – over 80% of our sample. These four industries also yield the most stable estimates, as evidenced by relatively small standard errors compared to the other seven industries. For both reasons we focus our initial discussion on these results and return to the small expenditure industries in the next section.

The first row of Table 1 presents the results allowing for plant-level, fixed effects. While none of the estimated nonenvironmental offsets is statistically different from zero, there is considerable variation even among these four, relatively well-behaved industries. The estimate for the plastics industry is that a one dollar increase in PACE expenditures leads to an 80 cent cost *savings* in nonenvironmental production costs. For steel a one dollar increase in PACE expenditures leads to an additional 41 cent cost *increase* in nonenvironmental production costs. Petroleum, which has the largest share of reported regulatory expenditures, shows a twenty-two cent savings while pulp and paper shows a thirty-six cent savings.

Even though these changes are not statistically significant, they indicate a potential for environmental expenditures to induce economically significant savings offsets. The possibility of large hidden costs, on the order of several times reported costs, is also ruled out. This result runs counter to previous econometric studies.

In order to further aggregate the four large expenditure industry results into a single estimate, we can again apply Equation (5). We use estimates of environmental spending by industry in 1994 – the most recent year available – to weight each industry's marginal cost estimate.[31] This

[29]This scheme preserves the computed elasticity α_r so it continues to reflect the aggregate, industry-level elasticity.

[30]In our formulation with a constant elasticity α_r, this reduces to $O_{agg} = \alpha_r \cdot (\sum_{i,t} PC_{i,t})/(\sum_{i,t} R_{i,t})$. However, we actually weight by real reported environmental expenditure, deflating by the GDP deflator. We do this so that earlier years are not discounted simply because of inflation. This corresponds to multiplying the estimated elasticity α_r by the ratio of real non-environmental production costs to real reported environmentalexpenditures, both summed across all observations, rather than nominal values of each.

[31] That is, we assume that incremental regulation is proportional to the level of reported expenditures in 1994 by

approach yields an aggregate estimate of nonenvironmental offsets of eighteen cents for every dollar of increased reported regulatory expenditures, with a standard error of forty-two cents.[32] Thus, our best estimate of the economic cost of a dollar increase in PACE expenditures is only eighty-two cents (one dollar minus eighteen cents). Based on a 95% confidence interval, the true economic cost ranges from negative two cents to positive $1.68. While this confidence interval is quite large, it again indicates that extremely large values ($> \$1.68$), are unlikely.

In contrast to the first row of estimates in Table 1 – which is based on the fixed effects model – the second row of estimates is based on a pooled model. Unlike the fixed effects approach, the pooled model assumes that the nonenvironmental offsets are completely explained by the included right-hand side variables. This means that the effect of any omitted variables will be attributed to the included right-hand side variables. This confounding of different effects potentially biases the environmental offset estimates.

Interestingly, the pooled estimates are higher than the fixed-effects estimates for all four industries, significantly so in petroleum and steel. The (weighted) average environmental offset, driven heavily by large increases in the petroleum and steel industries, rises to positive $1.73 per dollar of PACE expenditures, with a standard error of $0.34. Based on this pooled model, the total economic costs of a marginal dollar of reported environmental expenditures is $2.73 ($1.00 plus $1.73). The pooled estimate is not only much higher, it is also in line with previous estimates concerning the cost of regulation. Joshi et al. (1997) report an estimate of $7-12 for the steel industry using a cost function modeling approach. Based on a growth accounting model, Gray and Shadbegian (1994) find marginal costs of $1.74, $1.35 and $3.28 for paper mills, oil refineries and steel mills, respectively. Both studies pool their data, although Gray and Shadbegian (1994) also report results for a fixed-effects model that, like our fixed-effects results, are uniformly lower.[33]

industry. Within each industry incremental regulation is allocated to each observation in our multi-year sample in proportion to that observation's real regulatory expenditure.

[32] Note that with a weight of $0.430/0.816$, petroleum plays a key role in determining the aggregate estimate.

[33] $0.55, $0.97, and $2.76 for paper mill, oil refineries and steel mills, respectively, all of which are insignificantly different from both zero and one. They downplay these results based on the argument that measurement error is a bigger problem for the fixed-effect estimates than the pooled model. We disagree with this argument, as explained in Section B.2.

The implication of this comparison between the fixed-effects and pooled models is striking. Comparing differences *among* plants based on the pooled model, there appear to be additional costs associated with environmental protection but not included in PACE. Going back to Figure 1, this is analogously reflected by the pooled regression line which exhibits a positive slope. These additional costs generate a more than dollar-for-dollar increase in total costs for any change in reported environmental expenditures. However, such a comparison potentially ignores other important differences which exist among plants and which could confound such a measurement. If we instead control for these differences, estimate a fixed-effects model, and examine how changes in PACE expenditures for *a given plant* lead to changes in non-environmental costs for that plant, we find no evidence of a positive relationship. This is analogous to the fixed-effects regression line in Figure 1 which is almost flat. In our view, controlling for these omitted variables provides a more reliable measure of the true marginal cost. We therefore interpret these results as an indication that PACE expenditures, while generally accurate, may modestly *overstate* the cost of environmental regulation.

4.3 Small Expenditure Industries

We summarize the results for small expenditure industries in Table 2. Only one industry in this group (motor vehicles) accounts for more than 5% of total PACE expenditures in our group of eleven industries. As a group all these small industries account for only 18% of reported environmental expenditures in our group of eleven industries. The estimated nonenvironmental offsets for individual industries, which range from $-\$9.80$ to $\$25.32$, exceed any plausible range one might imagine to be accurate. Simultaneously, the standard errors are much higher – an average of $2.04 versus $0.42 for the large expenditure industries.

At the same time, these industries all have much smaller sample sizes compared to those in Table 1. They also include industries, e.g., semiconductors and pharmaceuticals, which may not yield to traditional production function modeling. In these industries, the assumption of a single, fairly homogenous output is challenged by the variety of products. Individual plants may be more

Table 2: Offset Estimates – Small Expenditure Industries
(standard errors are in parentheses)

	Industry:	malt beverages	printing	pharmaceuticals	refrigeration	semiconductors	motor vehicles	aircraft engines	cross-industry average	small and large average
	Full sample # of obs.	185	114	260	224	80	257	102		
①	fixed effect	–2.38 (1.31)	1.71 (3.17)	1.86 (1.24)	–2.39 (1.93)	3.16 (2.21)	25.32* (5.85)	–9.80 (8.49)	8.32* (2.04)	1.39 (0.51)
②	pooled	–2.66* (1.27)	6.72 (3.50)	3.64* (1.38)	7.55* (2.48)	13.55* (3.59)	27.80* (5.64)	30.40* (7.80)	13.96* (2.02)	3.98* (0.46)
	weight†	0.026	0.019	0.033	0.010	0.026	0.060	0.009	0.184	

†Ratio of expenditures in each industry to eleven-industry total. This weight is used to compute cross-industry averages. See footnote 31 in the text concerning calculation of the weights.
*Significant at the 5% level.

differentiated than their common four-digit SIC code suggests. Such issues make our cost function approach somewhat questionable when applied to these industries.

Another important distinction is between industries primarily investing in end-of-pipe treatment versus those which rely more on process changes to comply with environmental regulation. End-of-pipe expenditures, where pollution control occurs *after* the production process, are likely to be much easier for plants to measure. In the case of process changes, where pollution control occurs by changing the mix of inputs or otherwise altering the productive process, it may be difficult to tease out the level of environmental expenditures compared to a no-regulation alternative. In particular, as the menu of manufacturing technologies changes in response to demand for cleaner processes, with the dirtier alternatives being eliminated, there may be greater difficulty estimating environmental expenditures relative to a zero regulation baseline. Since many of these small expenditure industries are process-change oriented, it is not surprising that the corresponding marginal cost estimates are more exotic.

While it would have been reassuring to see results in the small expenditure industries parallel those in the large expenditure industries, we find two useful messages in Table 2. First, some patterns remain: the pooled estimates remain higher than the fixed-effects estimates for all but one of the small expenditure industries (malt beverages), suggesting that omitted-variable bias continues to be a problem. Second, the wide-ranging and implausible estimates may be just another indicator of the poor quality of the underlying PACE data, only exacerbated by the small sample size. This is consistent with the hypothesis that there may not be a systematic relationship between reported environmental expenditures and additional economic costs/savings in some industries.

5 Conclusion

Most previous analyses find that reported environmental expenditures are likely to *understate* the true economic cost of environmental protection. In contrast, our results rule out any significant understatement and instead point to a modest though statistically insignificant *overstatement*. Using our preferred fixed-effects model, we estimate an aggregate savings of eighteen cents in conventional production costs for every dollar of reported incremental pollution control expenditures with a standard error of forty-two cents. This is equivalent to saying that a dollar increase in reported environmental expenditures raises total (environmental + non-environmental) production costs by eighty-two cents.

We observe economically large, though statistically insignificant, variation in our estimates among industries. We find that reported environmental expenditures tend to generate savings in conventional production costs in the petroleum refining, plastics, and pulp and paper industries. In the iron and steel industry they generate added burdens. This variation could be viewed as a consequence of acknowledged quality issues in the PACE data. Alternatively, the observed variation might explain why some firms and/or industries may believe that PACE understates the true cost of environmental protection even if, on average, PACE is roughly right or even overstates true costs.

An important finding in our work is that alternative assumptions about productivity differences among plants produce vastly different estimates of economic costs. We find that estimates based on an alternative, pooled model consistently show smaller savings and larger additional burdens than the fixed-effects model. Use of a pooled model generates an aggregate estimate of $2.73 in higher costs for every additional dollar of reported regulatory expenditure – versus $0.82 for the fixed-effects model. In contrast to the fixed-effects specification, the pooled specification assumes that unmodeled differences between plants (e.g., age, location, management style) are unrelated to either total costs or reported environmental expenditures – or that environmental regulation *causes* those differences. Although the higher, pooled estimates are more consistent with previous work, we believe they are biased by omitted variables characterizing differences among plants.

Previous work that found substantial understatement of regulatory costs has leaned on several explanations of those results. Reduced flexibility, the crowding out of new investments, new source bias or simply poor accounting are all plausible reasons why reported costs would understate actual costs.

Our own observation of possible overstatement leans on two possible explanations: production complementarities/economies of scope or, once again, poor accounting. A plausible complementarity story arises, for example, if shutting down a production line is a substantial expense. Then, it makes sense that plant managers would undertake non-environmental modifications alongside environmental ones in order to take advantage of the forced downtime. Since it is cheaper to do the two modifications together rather than separately, this represents economies of scope. If the cost of the downtime is entirely allocated to the environmental project, it would not be surprising to find savings in conventional production associated with the increased expense on environmental activities.

While we find no statistical evidence of either over- or understatement of the cost of environmental regulation, the range of values included in a reasonable confidence interval, from 100% overstatement to 70% understatement, is economically significant and deserves to be scrutinized further. Based on these results, it is fair to say that the current emphasis on better measurement of the benefits associated with environmental protection ought to be balanced with greater attention to uncertainties about costs.

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A Data Sources

The data used in this paper are drawn from several plant-level datasets developed by the U.S. Census Bureau:

- *The Longitudinal Research Database (LRD)*. This is a pooled, cross-section, time series comprised of the establishment responses to the Annual Survey of Manufacture (ASM) and the quinquennial Census of Manufactures (CM) for over 50,000 establishments in each year. The LRD contains information on cost, outputs and inputs at the plant level. Detailed quantity and expenditure information for energy consumption are only available up to 1981.
- *The Manufacturing Energy Consumption Survey (MECS)*. Collected by the Department of Energy every three years beginning 1985, MECS contains detailed fuel consumption and expenditure data by establishment.
- *The Pollution Abatement Control Expenditure (PACE)*. This dataset includes pollution abatement investment spending and operating expenditures at the establishment level and has been collected by the Census Bureau for most years between 1979 and 1991, except 1983 and 1987.

For 11 four-digit SIC industries, our analysis includes the years 1979, 1980, 1981, 1985, 1988, and 1991. Sample sizes are shown in Table A.1.

Data on input and output quantities and prices are constructed as follows:

- *Output.* Data on the total value of shipments, by individual product codes, are contained in the LRD. We construct a divisia index of output price based on the corresponding producer prices of different product obtained from Bureau of Labor Statistics (Caves, Christensen, and Diewert 1982a; Caves, Christensen, and Diewert 1982b). The quantity index is obtained by dividing total value of shipments, adjusted for inventory, by this aggregate output price index.
- *Regulation.* Data on (nominal) annual pollution abatement operating costs at the plant level are from the annual Pollution Abatement Costs and Expenditure (PACE) Survey. Operating expenses for pollution abatement include depreciation on the pollution abatement capital. Detailed data are available for the years 1979-1991, except 1983 and 1987. Real regulatory expenditures are computed by deflating nominal pollution abatement operating costs by the GDP deflator.

Table A.1: Sample Size by Industry

Industry	Plants	Sample Size
Malt Beverage	45	185
Pulp and Paper	142	612
Printing	45	114
Plastic Material	107	403
Pharmaceutical	73	257
Petroleum	165	708
Steel	127	527
Refrigeration	76	224
Semiconductors	28	80
Motor Vehicles and Car Body	59	203
Aircraft Engine	29	102

- *Capital Stock.* The gross book value of the capital stock at the beginning of the year and new capital expenditures each year are reported in the LRD. Gross book value is used to compute the capital stock in 1979.[34] A perpetual inventory method (Christensen and Jorgenson 1969) is then used to generate a real capital stock series covering the period 1980-1991 based on the following formula:

$$k_t = (1-\delta)k_{t-1} + \frac{q_0}{q_t}I_t \tag{A.1}$$

where k_t is the period t capital stock and I_t is new capital expenditure measured in current dollars. The industry-specific economic depreciation rate (δ) is from Hulten and Wykoff (1981). The capital stock price indices (q_t) for various industries are drawn from a dataset developed by Bartelsman and Gray (1994).

- *Service Price of Capital.* The service price of capital is calculated using the Hall-Jorgenson (1969) procedure. The service price of capital is given by:

$$p_{k(t)} = [q_{t-1}r_t + \delta q_t - (q_t - q_{t-1}) + q_t C_t]\frac{1-u_t z_t - k_t}{1-u_t} \tag{A.2}$$

where,

[34] Specifically, capital stock is initialized to 0.45 times the gross book value in 1979. This ratio is based on the aggregate net asset to gross book value ratio computed in the steel industry where firm 10-Ks were available.

$$
\begin{aligned}
p_{k(t)} &= \text{ service price of capital,} \\
q_t &= \text{ price index of new capital equipment,} \\
r_t &= \text{ after tax rate of return on capital (opportunity cost),} \\
\delta &= \text{ rate of economic depreciation,} \\
C_t &= \text{ effective property tax rate,} \\
u_t &= \text{ effective corporate income tax rate,} \\
z_t &= \text{ present value of allowed depreciation tax deductions on a dollar's} \\
& \quad\ \text{investment over the life time of an asset,} \\
k_t &= \text{ investment tax credit,} \\
t &= \text{ year.}
\end{aligned}
$$

We use the average yield on Moody's "Baa" bonds for the after tax rate of return on capital. The data on the tax policy variables are from Jorgenson and Yun (1991) and Jorgenson and Landau (1993).

- *Capital Costs.* The capital costs were constructed as the product of the service price of capital and the stock of capital.

- *Labor.* The quantity of labor is defined as the number of production workers. The cost of labor includes production worker wages plus supplemental labor cost (which accured to both production workers and non-production workers) adjusted to reflect the production worker share. The price of labor is defined as the cost of production workers divided by the number of production workers.

- *Price of Materials.* Expenditure data on individual materials are collected on a five year cycle by the Census of Manufacturers (CM). We derive a divisia index of the price of materials for each plant for the years 1977, 1982, 1987, 1992. Estimates for intervening years are linearly interpolated.

- *Cost of Materials.* We use reported total expenditures on materials and parts in the LRD to calculate material costs.

- *Price of Energy.* Detailed data on total quantities consumed and total expenditures on various fuels were collected in LRD (through 1981) and MECS (1985, 88, 91). These data are used to calculate the prices of individual fuels ($/Mbtu) paid by each plant. The individual fuels include coal, natural gas, dfo, rfo, lpg and electricity. These fuels typically account for about 90 percent of total energy cost. The price of energy is computed as a divisia index of these fuels.

- *Cost of Energy* The cost of energy is the summation of expenditures for the six individual fuels.

Table B.2: Estimation results[a]
(standard errors are in parentheses)

Sector:	malt beverages	pulp and paper	printing	plastics	pharmaceuticals	petroleum	steel	refrigeration	semiconductors	motor vehicles	aircraft engines
α_y	0.7864*	0.7456*	0.7731*	0.8417*	0.4647*	0.6468*	0.7137*	0.7232*	0.4602*	0.7554*	0.7712*
	(0.0293)	(0.0278)	(0.0679)	(0.0356)	(0.0462)	(0.0383)	(0.0300)	(0.0252)	(0.0927)	(0.0214)	(0.0463)
α_r	–0.0167	–0.0109	0.0073	–0.0150	0.0247	–0.0026	0.0112	–0.0119	0.0347	0.0675*	–0.0257
	(0.0092)	(0.0078)	(0.0135)	(0.0105)	(0.0165)	(0.0091)	(0.0116)	(0.0096)	(0.0242)	(0.0156)	(0.0223)
α_t	–0.0097*	0.0109*	0.0286*	0.0124*	0.0383*	–0.0033	–0.0023	0.0024	–0.0409*	–0.0122*	0.0352*
	(0.0019)	(0.0011)	(0.0030)	(0.0020)	(0.0034)	(0.0020)	(0.0022)	(0.0025)	(0.0123)	(0.0024)	(0.0045)
β_{kk}	0.1020*	0.1047*	0.0425*	0.0501*	0.0660*	0.0101*	0.0693*	0.0067	0.0618	0.0033	0.0829*
	(0.0155)	(0.0172)	(0.0126)	(0.0119)	(0.0112)	(0.0018)	(0.0073)	(0.0049)	(0.0937)	(0.0036)	(0.0191)
β_{ll}	0.0273	0.1055*	0.0842*	0.0684*	0.1423*	0.0141*	0.0572*	0.0744*	0.3072*	0.0274*	0.1709*
	(0.0166)	(0.0116)	(0.0355)	(0.0092)	(0.0205)	(0.0014)	(0.0205)	(0.0150)	(0.0477)	(0.0139)	(0.0476)
β_{ee}	0.0180*	0.0693*	0.0093*	–0.0140	0.0192*	0.0112*	0.0340	0.0084*	0.0279*	0.0009	0.0058*
	(0.0034)	(0.0073)	(0.0038)	(0.0146)	(0.0078)	(0.0018)	(0.0182)	(0.0014)	(0.0081)	(0.0006)	(0.0029)
β_{yy}	–0.0678	–0.0215	0.2565*	–0.0343	0.0603	0.0283	0.0432*	–0.0292	0.1288*	0.1553*	0.0999*
	(0.0388)	(0.0322)	(0.0980)	(0.0335)	(0.0460)	(0.0236)	(0.0198)	(0.0223)	(0.0476)	(0.0248)	(0.0200)
β_{tt}	–0.0012	0.0032*	0.0004	–0.0006	–0.0007	–0.0036*	0.0002	0.0041*	–0.0034	–0.0105*	–0.0056*
	(0.0009)	(0.0006)	(0.0013)	(0.0010)	(0.0016)	(0.0009)	(0.0009)	(0.0011)	(0.0034)	(0.0011)	(0.0017)
β_{kl}	–0.0107	0.0053	0.0112	–0.0115	–0.0078	–0.0027*	–0.0113	0.0168*	–0.0323	–0.0132*	–0.0285
	(0.0105)	(0.0095)	(0.0101)	(0.0074)	(0.0103)	(0.0007)	(0.0083)	(0.0051)	(0.0442)	(0.0047)	(0.0219)
β_{ke}	–0.0012	–0.0229*	0.0009	0.0024	–0.0104	–0.0010	–0.0115	–0.0032*	–0.0067	–0.0001	–0.0016
	(0.0034)	(0.0080)	(0.0048)	(0.0077)	(0.0067)	(0.0011)	(0.0064)	(0.0014)	(0.0119)	(0.0005)	(0.0043)
β_{ky}	0.0048	0.0056	0.0060	–0.0393*	–0.0259*	–0.0110*	–0.0442*	–0.0180*	–0.0238	–0.0093*	–0.0144*
	(0.0072)	(0.0070)	(0.0066)	(0.0047)	(0.0068)	(0.0021)	(0.0030)	(0.0019)	(0.0177)	(0.0012)	(0.0064)
β_{kt}	0.0004	0.0020*	0.0029*	0.0013*	0.0040*	0.0014*	0.0001	0.0021*	0.0497*	0.0023*	0.0012
	(0.0008)	(0.0006)	(0.0008)	(0.0005)	(0.0009)	(0.0002)	(0.0004)	(0.0004)	(0.0067)	(0.0004)	(0.0013)
β_{le}	0.0072	–0.0159*	0.0027	–0.0079	–0.0068	0.0000	0.0220	–0.0052*	–0.0218*	–0.0013	–0.0078
	(0.0041)	(0.0058)	(0.0055)	(0.0061)	(0.0082)	(0.0010)	(0.0127)	(0.0018)	(0.0059)	(0.0007)	(0.0056)
β_{ly}	–0.0031	–0.0477*	–0.0294	–0.0299*	–0.0731*	–0.0051*	0.0032	–0.0146*	–0.0450*	–0.0340*	–0.0432*
	(0.0057)	(0.0053)	(0.0222)	(0.0039)	(0.0123)	(0.0010)	(0.0067)	(0.0048)	(0.0201)	(0.0032)	(0.0131)
β_{lt}	–0.0008	–0.0025*	–0.0099*	–0.0022*	–0.0067*	0.0002*	–0.0043*	–0.0036*	–0.0316*	0.0010	–0.0121*
	(0.0007)	(0.0005)	(0.0023)	(0.0005)	(0.0014)	(0.0001)	(0.0010)	(0.0008)	(0.0061)	(0.0009)	(0.0024)
β_{ey}	–0.0052*	–0.0061	–0.0081*	0.0063	–0.0084	–0.0081*	–0.0134	–0.0065*	–0.0070*	–0.0024*	–0.0105*
	(0.0014)	(0.0052)	(0.0034)	(0.0088)	(0.0051)	(0.0015)	(0.0080)	(0.0006)	(0.0033)	(0.0002)	(0.0015)
β_{et}	–0.0001	–0.0013*	–0.0002	0.0012	0.0020*	–0.0001	–0.0003	0.0002*	0.0028*	0.0002*	0.0000
	(0.0002)	(0.0004)	(0.0004)	(0.0008)	(0.0006)	(0.0002)	(0.0010)	(0.0001)	(0.0010)	(0.0001)	(0.0003)
β_{yt}	–0.0060*	0.0040*	–0.0001	0.0113*	0.0124*	–0.0020	–0.0002	–0.0031	–0.0045	–0.0146*	0.0106*
	(0.0024)	(0.0014)	(0.0043)	(0.0020)	(0.0029)	(0.0016)	(0.0015)	(0.0023)	(0.0088)	(0.0026)	(0.0022)

Table B.2: Estimation results[a] (continued)
(standard errors are in parentheses)

Sector:	malt beverages	pulp and paper	printing	plastics	pharmaceuticals	petroleum	steel	refrigeration	semiconductors	motor vehicles	aircraft engines
γ_k	0.8968*	0.1296	1.0556*	0.0832	–0.1918	0.0418	0.0922	0.7265*	–8.0098*	1.4184*	4.8810*
	(0.4143)	(0.1018)	(0.5235)	(0.0979)	(0.2658)	(0.0493)	(0.0634)	(0.3602)	(2.1659)	(0.3462)	(0.9041)
γ_l	1.2120*	0.1417	5.7061*	0.4864*	1.3077*	0.0913*	0.0655	2.9809*	8.4050*	0.2101	3.0846
	(0.3285)	(0.0745)	(1.7722)	(0.0802)	(0.4664)	(0.0271)	(0.1410)	(0.7965)	(2.1530)	(0.9009)	(1.6987)
γ_e	0.0769	0.2281*	0.2042	0.4369*	0.3105	0.0192	–0.4629*	–0.1840	–0.0905	0.2820*	0.2519
	(0.0913)	(0.0791)	(0.2763)	(0.1854)	(0.2062)	(0.0349)	(0.1700)	(0.1056)	(0.3224)	(0.0508)	(0.2201)
δ_{kk}	0.1833	0.8884	3.3909	0.3402	1.5320*	0.3015*	0.4278*	5.5208*	12.5426	2.0054	–6.2041*
	(1.5393)	(0.4830)	(2.6223)	(0.4753)	(0.7347)	(0.0946)	(0.2151)	(1.3693)	(9.3539)	(1.1609)	(3.0151)
δ_{kl}	1.9030	0.0100	–2.7094	0.0564	–0.3278	0.0348	–0.1817	–3.1469*	–2.9619	1.0930	1.8625
	(1.0765)	(0.2620)	(1.5152)	(0.2881)	(0.6966)	(0.0386)	(0.2587)	(0.6583)	(4.0159)	(1.2555)	(3.9295)
δ_{ke}	0.0160	–0.1390	1.2362	0.2610	0.0054	0.1158*	0.3625*	0.9717*	0.1633	–0.4050*	–0.6694
	(0.3295)	(0.1900)	(0.9237)	(0.2590)	(0.4071)	(0.0534)	(0.1811)	(0.3732)	(1.1747)	(0.1525)	(0.6523)
δ_{kt}	–0.0360	0.0011	–0.1320	0.0623*	0.1164*	–0.0031	–0.0099	–0.0794	–1.7104*	–0.0491	0.8046*
	(0.0794)	(0.0164)	(0.1299)	(0.0184)	(0.0368)	(0.0105)	(0.0159)	(0.0589)	(0.5190)	(0.0947)	(0.2100)
δ_{ll}	0.0787	–0.3325	–14.5991*	–1.0411*	–3.0608*	–0.2667*	1.4014*	3.4572*	–19.4742*	–3.5754	–28.8300*
	(1.9911)	(0.3182)	(4.9492)	(0.3346)	(1.3563)	(0.0989)	(0.6246)	(1.6408)	(4.0775)	(3.4818)	(10.1941)
δ_{le}	0.1147	–0.0890	–0.5959	0.3905	–0.1919	0.0116	–0.7759*	–0.0527	0.3539	–0.1234	3.5142*
	(0.4749)	(0.1332)	(0.7843)	(0.2105)	(0.5300)	(0.0372)	(0.3666)	(0.2197)	(0.5547)	(0.1874)	(1.2508)
δ_{lt}	0.1540*	–0.0135	0.7632*	0.0431*	0.0163	0.0088	–0.0748*	0.2831*	1.6125*	0.1938	1.1113*
	(0.0633)	(0.0125)	(0.3442)	(0.0174)	(0.0643)	(0.0067)	(0.0364)	(0.1252)	(0.4635)	(0.2180)	(0.4176)
δ_{ee}	0.1641	0.3831*	–0.6590	0.5664	0.8957*	–0.1164	–0.5289	–0.2840	1.3083*	1.1883*	0.9118
	(0.2947)	(0.1548)	(0.6229)	(0.4880)	(0.4269)	(0.0655)	(0.5056)	(0.2821)	(0.5362)	(0.1988)	(0.8036)
δ_{et}	–0.0272	–0.0228*	–0.0362	–0.0699*	–0.0103	–0.0083	0.0623	–0.0251	–0.1135	–0.0032	–0.1241*
	(0.0169)	(0.0101)	(0.0606)	(0.0265)	(0.0296)	(0.0073)	(0.0334)	(0.0193)	(0.0731)	(0.0129)	(0.0541)
observations	185	615	114	404	260	717	536	224	80	257	102
firms	45	142	45	107	73	165	128	76	28	59	29
R^2 total costs	0.99	0.98	0.99	0.98	0.98	0.97	0.98	1.00	0.98	0.99	0.99
R^2 capital share	0.70	0.80	0.98	0.81	0.85	0.75	0.80	0.90	0.88	0.75	0.87
R^2 labor share	0.86	0.84	0.93	0.91	0.84	0.86	0.80	0.96	0.87	0.85	0.94
R^2 energy share	0.81	0.87	0.92	0.70	0.76	0.87	0.49	0.94	0.91	0.91	0.93
log-likelihood	1951	5107	1280	3316	1964	7953	3699	2818	644	3278	1094
vs pooled[b]	449	1831	530	1155	844	1910	1186	931	263	640	401
vs C-D (PC)[c]	37	103	11	40	42	83	51	43	24	9	16
vs C-D (R)[d]	2	6	7	10	7	12	10	25	14	21	11

*Significant at the 5% level.
[a]Estimation is based on maximum likelihood, iterating on the cross-equation covariance matrix until it converges.
[b]Pooled estimation assumes $\alpha_i = \alpha \; \forall i$ in Equation (2). The value shown is the difference between log-likelihood values and, when multiplied by two, is an asymptotically chi-squared test of this hypothesis, with degrees of freedom equal to four times the number of plants minus one. This test rejects in all industries.
[c]C-D (PC) assumes $\beta_{kk} = \beta_{kl} = \beta_{ke} = \beta_{ll} = \beta_{le} = \beta_{ee} = 0$ (5% critical value is roughly 6.3).
[d]C-D (R) assumes $\delta_{kk} = \delta_{kl} = \delta_{ke} = \delta_{ll} = \delta_{le} = \delta_{ee} = 0$ (5% critical value is roughly 6.3).

B Estimation

Table B.2 presents detailed estimation results for the eleven industries considered in this study using the fixed-effects model discussed in Section 3.3. Parameter estimates for the 30 free slope parameters in Equation (3), standard errors, goodness of fit and likelihood statistics are reported. The difference in the log-likelihood relative to three alternative models is also provided.

Over half of the estimated parameters are significant at the 5% level. Many of the second order price coefficients are among the significant parameter estimates, both for the conventional production function (β) and the environmental abatement cost function (δ). It should not be surprising, then, that the tests of unit elasticity of substitution are rejected twenty-one out of twenty-two times.

The test that the model reduces to a pooled form, where all the fixed effects are the same, is also strongly rejected. In every industry the test difference in likelihoods is on the order of ten times the number of firms, whereas the critical value is on the order of four times the number of firms, divided by two.

These tests suggest that the flexible functional form being estimated cannot be reduced in a substantial way without significantly reducing the model's fit. The goodness of fit statistics indicate that large fraction of the observed variation is captured by our model. Much of this, especially in the share equations, is accounted for by the fixed effects.

Table B.3 provides additional information about the consistency of the estimated share values, fitted share values and own-price demand elasticities in light of economic theory. We first compare the observed cost shares to values found in both aggregate data (U.S. Department of Commerce) and another microeconomic study (Hazilla and Kopp 1990). Our estimates generally fall between the two estimates reflecting the fact that the historical scope of our data lies between the the more recent aggregate data and the older Hazilla and Kopp study.

Next we examine whether the fitted cost shares (e.g., factor demands) are positive and whether the own-price elasticities are negative.[35] Only a few of the fitted cost shares turn out to be negative. This typically occurs in those industries where one or more shares is near zero (petroleum, for

[35] This is less restrictive than the concavity required by economic theory but is simpler to verify.

example, where materials account for over 90% of costs). However, a large number of the own-price elasticities are positive, especially capital. This means that the factor demand schedules are locally upward sloping in many cases, contradicting economic theory.

A commonly acknowledged problem with the translog cost function is it's inability to accurately capture relatively inelastic factor demand (Perroni and Rutherford 1996). Because the elasticity varies with the factor shares, an average own-price elasticity near zero will imply that many of the locally evaluated elasticities will be positive. This is in fact what we observe.

By estimating an alternative Cobb-Douglas model, we verify that the observed positive own-price elasticities do not have important consequences for our primary results. This model restricts the factor demands by imposing an own-price elasticity of -1. This specification leads to an aggregate[36] estimate of the offset associated with environmental expenditures of 0.28, larger but insignificantly different from the translog estimate of -0.18. Since the Cobb-Douglas restriction is strongly rejected based on the log-likelihood test, we continue to focus our attention on the unrestricted model.

B.1 Distinguishing Environmental and Non-Environmental Cost Shares

Our econometric model is novel in that we estimate a cost function associated solely with production of conventional output Y alongside factor shares associated with the production of both Y and abatement effort. We are unable to estimate a cost function associated with abatement because we are unable to control for the scale of abatement, which is unobserved. Also, the raw data do not allow us to distinguish between those factor inputs used for abatement and those used for conventional production, making it impossible to estimate share equations associated solely with conventional production. However, the model specified in (3) circumvents this problem, distinguishing econometrically between factors used for environmental and non-environmental production.

To understand how environmental and non-environmental cost shares are identified, it is useful

[36]E.g., across the four large expenditure industries: pulp and paper, plastics, petroleum and steel.

Table B.3: Assessment of Model Consistency

	Industry:	malt beverages	pulp and paper	printing	plastics	pharmaceuticals	petroleum	steel	refrigeration	semiconductors	motor vehicles	aircraft engines
Comparison of average value shares												
Table B.2 estimates[a]	capital	0.056	0.092	0.059	0.063	0.074	0.020	0.060	0.026	0.136	0.015	0.047
	labor	0.141	0.201	0.359	0.085	0.238	0.019	0.230	0.236	0.370	0.118	0.333
	energy	0.028	0.120	0.024	0.057	0.032	0.022	0.104	0.016	0.046	0.006	0.022
	material	0.774	0.587	0.558	0.794	0.657	0.939	0.606	0.723	0.448	0.861	0.598
1987 I-O tables[b]	capital	0.131	0.153	0.166	0.173	0.302	0.105	0.074	0.099	0.166	0.063	0.067
	labor	0.177	0.265	0.350	0.180	0.314	0.071	0.299	0.366	0.387	0.123	0.414
	energy	0.014	0.052	0.012	0.049	0.014	0.021	0.095	0.012	0.019	0.008	0.011
	material	0.678	0.530	0.471	0.598	0.370	0.803	0.532	0.523	0.428	0.805	0.508
Hazilla and Kopp[c]	capital	0.020	0.066	0.054	0.059	0.071	0.055	0.061	0.032	0.166	0.032	0.021
	labor	0.116	0.202	0.362	0.212	0.146	0.039	0.258	0.292	0.387	0.231	0.298
	energy	0.013	0.046	0.009	0.040	0.112	0.037	0.034	0.016	0.019	0.010	0.013
	material	0.852	0.685	0.574	0.689	0.672	0.870	0.647	0.660	0.428	0.727	0.668
Fraction of observations with negative estimated share values (zeros are omitted)[d]												
	capital	0.005	0.005	0.018	0.002	0.004	0.060	0.024	0.027	0.038	0.101	0.069
	labor				0.007	0.004	0.001			0.025		
	energy		0.002		0.025	0.027	0.007					
	material											
Fraction of observations with positive own-price elasticities (zeros are omitted)[d]												
	capital	0.968	0.730	0.526	0.436	0.485	0.287	0.692	0.121	0.288	0.218	0.971
	labor		0.037		0.559	0.277	0.389	0.007		1.000		0.176
	energy	0.070	0.171	0.061	0.010	0.415	0.237	0.032	0.134	0.188	0.004	0.069
	material	0.184	0.130	0.035	0.077	0.200	0.138	0.054	0.013	1.000		0.137

[a] Average of the observed share values in each industry.

[b] Energy, materials and value added expenditures are from the 1987 Benchmark Input-Output Tables, U.S. Department of Commerce (April, 1994). Further breakdown of value added is based on a more detailed version of Gross Product Originating data, U.S. Department of Commerce (August, 1996).

[c] From Hazilla and Kopp (1986).

to consider a simpler model where production costs are given by

$$PC = \alpha_0 + \alpha_k \log P_k + \alpha_l \log P_l + \alpha_e \log P_e + \alpha_m \log P_m$$

and the aggregate shares by

$$v_k = \frac{R}{R + PC}\gamma_k + \frac{PC}{R + PC}\alpha_k$$

etc. Here, the translog specifications have been simplified to Cobb-Douglas and all plant, time and scale effects have been suppressed. The share equations can be rewritten

$$v_k = \alpha_k + \frac{R}{R + PC}(\gamma_k - \alpha_k) \tag{B.3}$$

Based on (B.3) one way to view the distinction between cost shares is how variation in the level of regulatory expenditures leads to different aggregate factor shares, all else equal. Suppose, for example, that no abatement occurs and $R/(R + PC) = 0$. The observed cost share of capital would then reveal the value of α_k, the underlying non-environmental cost share. In contrast, if no production of conventional output Y occurs and $R/(R + PC) = 1$, the observed cost share of capital would reveal the value of γ_k, the underlying environmental cost share. While we are unlikely to observe such extreme cases, variation in $R/(R+PC)$ coupled with our specific modeling assumptions are sufficient to distinguish environmental from non-environmental factor inputs.

If the ratio of regulatory expenditures to total costs, $\frac{R}{R+PC}$ is constant, this method of identification will not work.[37] That is, if $\frac{R}{R+PC}$ is constant, it becomes colinear with the constant term in (B.3). However, jointly estimating the production cost model with the share equations continues to identify all the parameters even in this case. Specifically, the α's will be identified by the cost/price elasticities in the cost function and the γ's will be identified by the difference betweeen those cost/price elasticities and the observed aggregate share values. In other words, as input prices vary, changes in non-environmental production costs PC identify the α parameters. Descrepancies between these values and the observed cost shares then reveal the γ parameter values.

The standard motivation for estimating the share equations jointly with the cost function is to improve efficiency by providing multiple relations (cost function and share equations) that involve

[37]In the petroleum sector, this ratio varies from less than 0.01% to slightly more than 11.0% with a mean of 1.1% and standard deviation of 1.2%.

only those parameters in the original cost function. Under the aforementioned circumstances, however, this gain is nullified by the additional parameters that appear in the share equations (γ's). It is therefore possible that this effort to estimate multiple equations will be no more efficient than the estimation of a single cost function equation. These circumstances require (a) a lack of variation in $R/(R + PC)$, the share of reported regulatory expenditures in total costs, and (b) additional parameters γ that appear one-for-one with each α appearing in the share equations.[38] To the extent that these conditions are *not* satisfied, there continue to be efficiency gains from joint estimation.

B.2 Parameter Validity

Since our goal is to use the estimated parameters to compute the cost of environmental expenditures, the validity of these parameter estimates is critical.[39] Our estimates hinge on the following key assumptions:

1. The translog cost function given in Equation (2) provides a reasonable local approximation to the true underlying cost function.

2. The data are measured accurately.

3. Input prices and the level of output are fixed prior to the plant's production decisions each period.

There are, of course, grounds to question each of these assumptions. The first assumption deals with specification: if the model is misspecified, the parameter estimates will be biased. More to the point, the parameters are no longer structural in the sense of revealing the true nature of a production technology. However, this is a criticism with little practical import since our model is more flexible than other models which have been applied to the same problem. For the industries with smaller samples discussed in Section 4.3, one might argue that our specification is *too* flexible.

[38]If, for example, the model for PC were translog and the model for environmental costs were Cobb-Douglas, the number of additional parameters γ would be smaller than the number of original parameters α, reducing this effect.

[39]The consistency of the estimated parameters with economic theory, e.g., positive and downward sloping factor demand, is explored in Table B.3 and discussed in the Appendix.

The second assumption raises the issue of measurement error in the data – a problem widely acknowledged with regard to PACE data. However, since we want to relate reported expenditures to actual costs, whether or not reported expenditures accurately reflect some well-defined cost concept is not particularly important. Our concern that reported expenditures might be a poor indicator of related changes in production costs actually *hinges* on the presence of some measurement error. To the extent that this measurement error might be exacerbated by our fixed-effects approach – an issue often cited in discussions of panel data (Griliches 1979) – there are several countervailing points.[40] The first is that the exacerbation of measurement error bias in fixed-effects estimates hinges on the assumption that the measurement error is less correlated across observations from the same plant than the underlying regressor (true regulatory expenditures). If instead the measurement error is more correlated – as might arise if variation in plant accounting is the major source of error – a fixed-effects model *removes* measurement error.[41] Second, even if there is a bias, it is unclear which direction it goes. With both positive and negative estimates of non-environmental offsets, a bias towards zero has ambiguous results in the aggregate. Finally and most importantly, any concern about measurement error must be balanced against the possibility of omitted variable bias. Given the long list of potential confounding variables which could affect both reported regulatory expenditures and total costs, the risk of falsely attributing such effects to reported expenditures leads us to side strongly with the fixed-effects model.

The last assumption deals with the issue of endogeneity. Specifically, we are interested in the effect of increased environmental expenditures – presummably caused by tighter regulations – on non-environmental expenditures. If changes in non-environmental expenditures instead affect environmental expenditures, or if some omitted variable besides regulation affects both, the esti-

[40]Measurement error in a given variable biases that coefficient estimate towards zero. See Section 9.5 of Greene (1990) regarding the general issue of bias due to measurement error and Chamberlain (1984) or Hsiao (1986) regarding the issue of measurement error in panel data.

[41]Gray and Shadbegian (1994) make exactly the opposite point: if measurement error has nothing to do with differences between plants, using a fixed-effects model will serve only to remove a large fraction of the real underlying variation in data, e.g., between plants. This leaves measurement error as a larger percentage of the remaining within-plant variation. Such an effect would tend to favor the random-effects or pooled models, where between plant variation is preserved. We see no reason, however, to believe that measurement error is less related to plant differences than the true variation.

mated parameters may be biased.[42] For example, suppose that downtime is a significant expense and that plants have a number of planned production upgrades which remain unimplemented because of these downtime costs. When it becomes necessary to stop production to install additional abatement equipment, these queued projects might be then simultaneously undertaken. This "harvesting" of non-environmental projects introduces a negative correlation between between environmental expenditures and annual production costs – especially if the full cost of the downtime is attributed to environmental expenditures and the non-environmental projects are productivity enhancing.

Whether such a decrease in production costs is attributable to increased environmental expenditures depends on whether the non-environmental improvements would, in fact, have been implemented anyway. If so, we would have to view increased environmental expenditures as an endogenous outcome of the decision to shut down production (made by the plant), rather than an exogenous consequence of tighter regulation (made by some external authority). Deily and Gray (1991) have gone even further, arguing that the level of external regulatory stringency and enforcement is itself sensitive to productivity shocks (e.g., production costs), potentially making environmental expenditures endogenous even if they are beyond the control of the plant.

There is no easy way to deal with this potential endogeneity. On the one hand, there has been considerable exogenous variation over time in regulatory stringency.[43] This only indirectly addresses the issue, however, by suggesting the endogenous response is likely to be small. On the other hand, instrumental variables which might be used to isolate only exogenous changes in regulation are likely to vary only across plants.[44] As pointed out in Section 3.3, using variation across plants to estimate the effect of regulation is itself problematic because of omitted plant-specific variables. It is our belief that omitted-variables bias is the more significant of these two

[42] In particular, we need environmental expenditures to be *predetermined* (see Engle, Hendry, and Richard 1983).

[43] The level of stringency of air, water, and waste regulations increased significantly over the period of our data (1979-91). In (real) dollar terms, total environmental expenditures (with investment outlays annualized at a 7% discount rate) have doubled over the period (U.S. Environmental Protection Agency 1990) while reported annual PACE expenditures have increased by 50%.

[44] For example, the instruments used by Gray and Shadbegian (1994) – activeness of state enforcement, fuel use, inclusion in a non-attainment area – are all primarily, if not exclusively, plant-specific characteristics.

problems.[45] In Section 4.2 we see that this omitted-variable bias is potentially quite large.

[45]Gray and Shadbegian (1994) test the exogeneity of regulatory expenditures with respect to output and fail to reject that hypothesis. The relevance of the their test to our predicament is unclear, however, since they are using a different dependent variable and are focused on cross-section variation, which we ignore.