# Are We Overstating the Economic Costs of Environmental Protection?

Richard D. Morgenstern
William A. Pizer
Jhih-Shyang Shih

Discussion Paper 97-36

May 1997

Resources for the Future
1616 P Street, NW
Washington, DC 20036
Telephone 202-328-5000
Fax 202-939-3460

## Are We Overstating the Economic Costs of Environmental Protection?

Richard D. Morgenstern, William A. Pizer, and Jhih-Shyang Shih

### Abstract

Reported expenditures for environmental protection in the U.S. are estimated to exceed $150 billion annually or about 2% of GDP. This estimate is often used as an assessment of the burden of current regulatory efforts and a standard against which the associated benefits are measured. This makes it a key statistic in the debate surrounding both current and future environmental regulation. Little is known, however, about how well reported expenditures relate to true economic cost. True economic cost depends on whether reported environmental expenditures generate incidental savings, involve uncounted burdens, or accurately reflect the total cost of environmental protection.

This paper explores the relationship between reported expenditures and economic cost in a number of major manufacturing industries. Previous research has suggested that an incremental $1 of reported environmental expenditures increases total production costs by anywhere from $1 to $12, i.e., increases in reported costs probably *understate* the actual increase in economic cost. Surprisingly, our results suggest the reverse, that increases in reported costs may *overstate* the actual increase in economic cost.

Our results are based a large plant-level data set for eleven four-digit SIC industries. We employ a cost-function modeling approach that involves three basic steps. First, we treat real environmental expenditures as a second output of the plant, reflecting perceived environmental abatement efforts. Second, we model the joint production of conventional output and environmental effort as a cost-minimization problem. Third, we calculate the effect of an incremental dollar of reported environmental expenditures at the plant, industry, and manufacturing sector levels. Our approach differs from previous work with similar data by considering a large number of industries, using a cost-function modeling approach, and paying particular attention to plant-specific effects.

Our preferred, fixed-effects model obtains an aggregate estimate of thirteen cents in increased costs for every dollar of reported incremental pollution control expenditures, with a standard error of sixty-one cents. This single estimate, however, conceals the wide range of values observed at the industry and plant level. We also find that estimates using an alternative, random-effects model are uniformly higher. Although the higher, random-effects estimates are more consistent with previous work, we believe they are biased by omitted variables characterizing differences among plants.

While further research is needed, our results suggest that previous estimates of the economic cost associated with environmental expenditures have been biased upward and that the possibility of overstatement is quite real.

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

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

# Contents



# List of Figures



# List of Tables

## Are We Overstating the Economic Costs of Environmental Protection?

Richard D. Morgenstern, William A. Pizer, and Jhih-Shyang Shih[1]

# 1 Introduction

Expenditures for environmental protection in the U.S. are estimated to exceed $150 billion annually, with about one-quarter of the total arising in the manufacturing sector. Dynamic analyses suggest that, because of consequent declines in capital accumulation, the long-term annual resource costs of environmental protection may be considerably higher. Little is known, however, about how the underlying data on environmental abatement expenditures – essentially self-reported accounting information collected by the Census Bureau – relate to the traditional notion of economic cost. Abatement expenditures in the manufacturing sector reflect expenses that the plant manager identifies with environmental protection. Yet, the cost to society depends on the consequent change in total production costs and output price. Reported increases in environmental expenditures at the plant level may or may not result in dollar-for-dollar increases in production costs. The change in production costs depends on whether an increase in reported environmental expenditures incidently saves money, involves uncounted burdens, or has no other consequence.[2]

Most research on this distinction between reported environmental expenditures and true 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

[1] Visiting Scholar, Fellow, and Research Consultant, respectively, Quality of the Environment Division, Resources for the Future. (Morgenstern is also Associate Assistant Administrator, on leave, U.S. Environmental Protection Agency). The authors gratefully acknowledge technical assistance from the Center of Economic Studies (CES), U.S. Bureau of the Census, and financial support from the U.S. Environmental Protection Agency (Cooperative Agreement No. 821821-01-4). Robert Bechtold, Arnold Reznek,and Mary Streitwieser at CES all provided helpful assistance. At Resources for the Future, Raymond Kopp has been a continuing source of advice and support. Dallas Burtraw, Winston Harrington, Richard Newell, and Paul Portney all provided helpful comments on an earlier draft. The authors alone are responsible for all remaining errors.

[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 true economic cost of regulation.

associated with environmental controls.

In contrast, more limited research has been conducted on the possibility that there is at least some complementarity between pollution control and other production activities. That is, the costs of jointly producing conventional output and a cleaner environment may be lower than if each were produced separately.[3] Although it seems unlikely that total costs could actually fall as a result of rising abatement expenditures, this possibility has been explored in the context of sub-optimal behavior by firms. 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 consensus in the economics literature is that an incremental $1 of reported environmental expenditures probably increases total production costs by more than $1. Recent studies – based on models involving pooled cross-section and time-series data – suggests that production costs may rise by as much as $12 for every $1 of reported expenditures.

Acting as Special Sworn Agents of the U.S. Census Bureau, we have merged several large data sets containing plant-level information on prices and quantities of both inputs and outputs. The sample consists of more than 800 different manufacturing plants for eleven 4-digit SIC industries over the period 1979-1991. To account for productivity differences among plants, we estimate a fixed-effects model and conduct sensitivity analyses to address potential data deficiencies. For comparison, we also estimate a random-effects model which is similar to the pooled model used by other researchers.

Our results are quite surprising. While we observe a great deal of variation across industries as well as across plants within an industry, we find that a $1 increase in reported abatement ex-

[3] Suppose a potential investment both increases efficiency and reduces pollution but is not quite competitive with 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.

penditures, on average, is likely to raise total production costs by less than $1. Our preferred specification leads to an estimate of thirteen cents in increased costs for every dollar of reported incremental pollution control expenditures, with a standard error of sixty-one cents. These results depend critically on the assumption of a fixed-effects model. Estimates using a random-effects model are uniformly higher – each dollar of increased reported expenditures is associated with, on average, $3.70 in increased social costs. Although the higher estimates are more consistent with previous work, we believe they are biased by omitted variables characterizing differences among plants.

## 2 Review of the Literature

The key issue addressed in this paper is the possible gap between a measure of true economic cost of environmental expenditures and the measure provided by readily available information. To obtain a direct estimate of true economic cost, we can imagine the Census Bureau asking plant managers 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 purhase of new equipment because of differential performance requirements for new versus existing equipment. Exclude expenditures related to occupational health and safety. When process changes (as opposed to end-of-the pipe additions) are involved, be sure to allocate only that portion of the costs attributable to environmental protection."

Few firms, of course, possess the information to answer such a complex and comprehensive question reliably. What is available is the Pollution Abatement Costs and Expenditures (PACE) Survey. Collected by the U.S. Census Bureau in most years since1973, 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.[4]

PACE results have been regularly published by Census and represent, by far, the most comprehensive source of information on environmental costs. They form the basis of calculations by the Environmental Protection Agency indicating that annualized environmental costs exceed $150 billion – more than two percent of GDP (U.S. Environmental Protection Agency 1990).[5] PACE data have been used as inputs in dynamic general equilibrium analyses to estimate the dynamic 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. Researchers have found that environmental regulations accounted for between 8 and 44 percent of the observed 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. Various studies 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 or 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,

[4]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 questionable whether survey respondents are able to determine what environmental expenditures would have been made in the absence of regulation.

[5]The EPA estimates are somewhat higher than those developed by Census Bureau largely because: 1) EPA annualizes investment outlays (at 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.

in turn, could also lead to higher costs (Joshi et al. 1997).

In contrast, items (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 clean 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 measure these so-called offsets but they are among the items thought to be most subject to measurement error (Streitweiser 1996).[6]

Item (c) represents the notion that because plants may not be operating at peak efficiency prior to the imposition of environmental requirements, significant opportunities may exist to lower costs. The underlying argument has its roots in the work of Leibenstein (1966) and others who have written about suboptimal firm behavior. The basic application to environmental issues goes back at least to Ashford, Ayers, and Stone (1985). The most recent and widely debated discussion is associated with Porter and colleagues (Porter 1991; Porter and Van der Linde 1995). Porter 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, he argues that the complementarities between environmental activities and conventional production (item b) combined with the induced innovations associated with environmental requirements (item c) can partially offset or actually exceed the direct expenditures associated with environmental protection (item a).

As pointed out by Palmer and Simpson (1993), there are a number of ways of interpreting Porter's argument. At its simplest level it can be taken to mean that some industries, e.g., environmental services, will benefit from stringent regulation. Alternatively, it can be taken to mean that

[6] On conceptual grounds we prefer to estimate the difference between pre-offset reported expenditures and true costs. However, as an experiment, we re-estimated the model presented in Section 3 (below) netting out the value of offsets reported in the PACE survey. The results are almost identical to those reported in the paper.

some firms or even some industries subject to strict regulation will benefit relative to others. For instance, Chrysler might have benefitted on an absolute basis as well as gained an edge over its competitors when auto fuel efficiency standards were imposed in the mid 1970's because its fleet was weighted towards smaller-sized models.

Proponents of the Porter hypothesis have, in fact, gone further than these interpretations and argued that the productivity of the economy as a whole can be enhanced by stricter regulation. Barrett (1992) has explored a series of models in which regulators and polluting firms behave strategically to improve the country's exports. Other studies explicitly incorporate R&D expenditures by firms (Simpson and Bradford 1996; Ulph 1994). Even in domestic markets, it has been suggested that stringent regulation can spur positive changes in the long term by encouraging innovative solutions.

Economists have been generally 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 (Ashford, Ayers, and Stone 1985; Palmer, Oates, and Portney 1995). 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). However, these examples are generally interpreted as special cases.

Actual plant-level responses to our hypothetical survey question would enable researchers to measure the relative importance of these various, often countervailing influences. Absent this ideal, hypothetical data, we can only estimate the net effect based on available PACE data. Several recent papers have attempted to do just that. Work by Gray (1987) and Gray and Shadbegian (1994) explored these issues in the context of growth accounting. Using a simple model where environmental expenditures and 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, in fact, 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.

As we will see in the next section, our approach is similar to the JLSM work in that we estimate a cost function with environmental expenditures treated as a fixed output. 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]

We distinguish ourselves from both studies, however, in our treatment of differences among plants. While we are able to replicate their general results in Section 4, we show that their results depend critically on the assumption of complete homogeneity among plants.[9] This assumption, which assumes that differences in plant location, age and management have no effect on either productivity or environmental expenditure, 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

[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 is depends on the level of inputs rather than the level of output.

[9] 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 4.1, this is not necessarily the case and, even if it were, there are other compelling reasons to prefer the results allowing for heterogeneity.

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 was randomly subject to higher regulatory standards. Using this data we could simply examine the difference in average total costs between the two populations and then compare it to the difference in average reported PACE expenditures. The ratio of the differences would reveal what fraction of higher reported costs translates into higher total costs of production. Since the two groups would be otherwise identical (due to randomization), this would yield an unbiased estimate of the relation.

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 total 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, management style, age, or other plant characteristics, could influence both the level of environmental expenditures and total costs. Simple pooling of the data to estimate the cost implications of higher 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 all between-plant variation in total costs will be ascribed to these fixed effects. Thus, the effect of more expensive regulation is estimated solely by examining changes in environmental expenditures *over time* associated with changes in total costs *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

This paper adopts a cost-function modeling approach to isolate the effect of regulatory expenditures on total costs. We do this in three steps. First, we treat real (i.e., inflation-adjusted) environmental expenditures as a second output reflecting perceived environmental abatement effort. Second, we model the joint production of conventional output and environmental effort as a cost minimization problem for the plant and estimate a set of structural parameters. Third, we use this estimated model to compute the effect of an incremental dollar of reported environmental expenditures on total costs at the plant, industry and manufacturing sector levels.

The first step, treating environmental expenditures as a second output, is a convenient way to formalize the presence of environmental expenditures as an explanatory variable. For estimation purposes, we need to assume that the level of environmental expenditure is fixed before other costs and factor demands are determined (see Section 4.1).[10] Given this assumption, viewing perceived abatement effort (i.e., environmental expenditures) as a second output simply allows us to use concepts like (dis)economies of scope, joint production and complementarity to describe the potential for increased costs or savings as environmental expenditures change.

This treatment also lends itself to structural modeling, i.e., attempting to capture technological constraints rather than simply looking for correlation – is useful in that it gives us greater confidence that the parameters are invariant to changes in regulatory expenditures. To estimate a structural model, we adopt a cost-function approach where output and prices are considered ex-

[10] Since environmental expenditure represents perceived costs, it is more sensible to talk about its effect on *remaining* total cost and factor demands. However, since we cannot distinguish between what the plant claims as environmental and non-environmental factor demands, we construct a model where total cost and total factor demands act as the dependent choice variables. The requisite assumption – that non-environmental factor demands and cost are determined after environmental expenditures – is the same.

ogenous, and the plant chooses the optimal combination of inputs. This, in turn, determines total costs. From the plant's perspective, conventional output is primarily a choice of the plant's owners, who decide on the scale of operations, and environmental expenditure is largely a choice of the regulatory authorities. This seems more plausible than a conventional production-function approach, which instead treats inputs as exogenous and outputs as endogenous.[11] To the extent that the required scale of regulatory expenditure is more closely tied to the levels of outputs rather than the levels of inputs, this could bias the parameter estimates upwards as environmental expenditures become endogenous with respect to output.

An important aspect of our modeling of production costs is the potential to capture economies of scope.[12] This gives us an idea, loosely speaking, of the possible complementarities involved in the joint production of conventional output and environmental expenditures. To the extent that environmental activities are completely disjoint from regular production, the marginal economic cost of an additional dollar of environmental expenditures should 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 regulatory expenditures and could lead to a marginal economic cost exceeding one dollar.

### 3.2 Specification

We now explain the technical details and assumptions of our model. In each period $t$ we assume each plant $i$ wishes to minimize the total cost associated with producing a given quantity of conven-

[11] In the Appendix, we also explore treating capital as a fixed input and *variable* cost as the objective of the plant. This does not have a significant effect on our results.

[12] See Bailey and Friedlaender (1982).

tional output $Y_{i,t}$ and a given amount of perceived environmental abatement effort $R_{i,t}$, measured by reported environmental expenditures.[13] Here the subscript $i$ indexes over plants and $t$ indexes over time. The function $F_t(\cdot)$ defines a production technology involving these two outputs coupled with inputs of capital, labor, energy, and materials. In particular, $F_t(\cdot) = 0$ is the production *frontier* and $F_t(\cdot) < 0$ describes feasible but inefficient input/output combinations.[14] The production function is indexed over time to allow for exogenous time trends in productivity. The cost minimization performed by the plant is given by:

$$TC_{i,t} = \min_{K_{i,t},L_{i,t},E_{i,t},M_{i,t}} P_{k,i,t}K_{i,t} + P_{l,i,t}L_{i,t} + P_{e,i,t}E_{i,t} + P_{m,i,t}M_{i,t} \tag{1}$$
$$\text{such that } F_t(Y_{i,t}, R_{i,t}, A_iK_{i,t}, A_iL_{i,t}, A_iE_{i,t}, A_iM_{i,t}) \leq 0 \text{ with } Y_{i,t} \text{ and } R_{i,t} \text{ fixed.}$$

where inputs of capital $K$, labor $L$, energy $E$ and materials $M$ in the function $F_t(\cdot)$ are scaled by a plant specific constant $A_i$ representing differences in plant productivity. $P_k$, $P_l$, $P_e$, and $P_m$ represent prices of capital, labor, energy and materials, respectively. Prices and quantities are allowed to vary across plants and time.

The minimization in (1) defines a cost function $TC_{i,t} = A_i \cdot G_t(Y_{i,t}, R_{i,t}, P_{k,i,t}, P_{l,i,t}, P_{e,i,t}, P_{m,i,t})$.[15] We specify this cost function to be of the translog functional form:[16]

$$\log(TC_{i,t}) = \log(A_i) + \log(G_t(\cdot)) = \alpha_i + \alpha_x' \cdot X_{i,t} + \frac{1}{2}X_{i,t}'\beta_x X_{i,t} \tag{2}$$

where $X_{i,t} = \{\log(Y_{i,t}), \log(R_{i,t}), \log(P_{k,i,t}), \log(P_{l,i,t}), \log(P_{e,i,t}), \log(P_{m,i,t}), t\}'$, $\alpha_x = \{\alpha_y, \alpha_r, \alpha_k, \alpha_l, \alpha_e, \alpha_m, \alpha_t\}'$, $\beta_x = \begin{bmatrix}\beta_y & \beta_r & \beta_k & \beta_l & \beta_e & \beta_m & \beta_t\end{bmatrix}$ and $\beta_y = \{\beta_{yy}, \beta_{yr}, \beta_{yk}, \beta_{yl}, \beta_{ye}, \beta_{ym}, \beta_{yt}\}'$, $\beta_r = \{\beta_{ry}, \beta_{rr}, \beta_{rk}, \beta_{rl}, \beta_{re}, \beta_{rm}, \beta_{rt}\}'$, etc. We impose symmetry $(\beta_{ij} = \beta_{ji})$ and homogeneity of degree one on prices. That is, a doubling of prices should exactly double total costs. Homogeneity generates 8 additional constraints and, coupled with the symmetry restrictions, leaves 27 free parameters to be estimated.

[13] Data sources for the different variables are given in the Appendix.

[14] 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.

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

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

We estimate Equation (2) simultaneously with equations specifying the share of total costs for each input derived by Shepard's Lemma:

$$\begin{aligned}
\frac{\partial \log(TC_{i,t})}{\partial \log(P_{k,i,t})} &= \frac{P_{k,i,t}K_{i,t}}{TC_{i,t}} = v_k = \alpha_k + \beta_k' X_{i,t} \\
\frac{\partial \log(TC_{i,t})}{\partial \log(P_{l,i,t})} &= \frac{P_{l,i,t}L_{i,t}}{TC_{i,t}} = v_l = \alpha_l + \beta_l' X_{i,t} \\
\frac{\partial \log(TC_{i,t})}{\partial \log(P_{e,i,t})} &= \frac{P_{e,i,t}E_{i,t}}{TC_{i,t}} = v_e = \alpha_e + \beta_e' X_{i,t} \\
\frac{\partial \log(TC_{i,t})}{\partial \log(P_{m,i,t})} &= \frac{P_{m,i,t}M_{i,t}}{TC_{i,t}} = v_m = \alpha_m + \beta_m' X_{i,t}
\end{aligned}$$

where $v_k$, $v_l$, $v_e$ and $v_m$ are the input cost shares for capital, labor, energy and materials, respectively, and $\beta_k = \{\beta_{ky}, \beta_{kr}, \beta_{kk}, \beta_{kl}, \beta_{ke}, \beta_{km}, \beta_{kt}\}'$, $\beta_l = \{\beta_{ly}, \beta_{lr}, \beta_{lk}, \beta_{ll}, \beta_{le}, \beta_{lm}, \beta_{lt}\}'$, etc. One of these four share equations is redundant by the price homogeneity restrictions and is therefore dropped from the estimation procedure.[17] Specifically, we add a vector of normal, independent and identically distributed stochastic disturbances to the cost function and three share equations, then estimate the following system:[18]

$$\begin{aligned}
\log(TC_{i,t}) &= \alpha_i + \alpha_x \cdot X_{i,t} + \frac{1}{2} X_{i,t}' \beta_x X_{i,t} + \epsilon_{1,i,t} \\
v_l &= \alpha_l + \beta_l' X_{i,t} + \epsilon_{2,i,t} \\
v_e &= \alpha_e + \beta_e' X_{i,t} + \epsilon_{3,i,t} \\
v_m &= \alpha_m + \beta_m' X_{i,t} + \epsilon_{4,i,t}
\end{aligned} \tag{3}$$

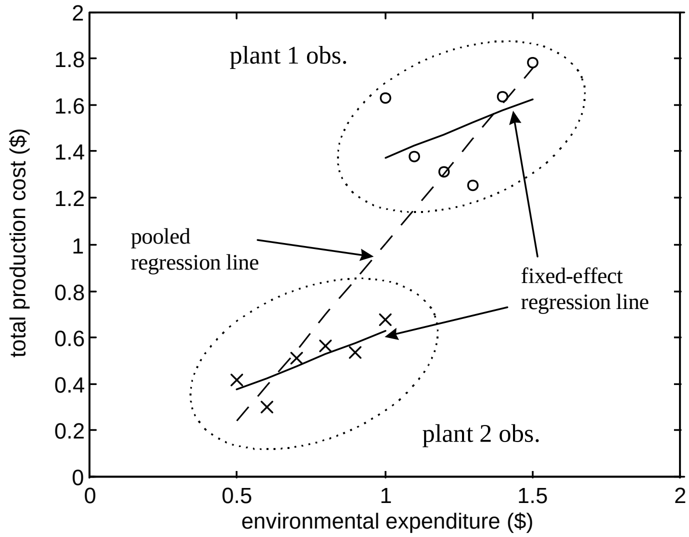
### 3.3 Accounting for Plant-Level Differences

The model derived in the previous section allows for plant-level differences. These differences are manifest in the parameter $\alpha_i$ in (2) and $A_i = \exp(\alpha_i)$ in (1), whereby some plants require proportionally more inputs to produce the same level of output. Within this specification, however, we still have considerable latitude to choose how $\alpha_i$ varies among plants.

[17] We omit the share equation for 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.

[18] We allow for contemporaneous correlation of the disturbance, e.g., between $\epsilon_{1,i,t}$ and $\epsilon_{2,i,t}$.

Figure 1: Fixed Effects versus Pooled Estimator




At one extreme, we could assume the $\alpha_i$ are all the same. 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 thereby ignoring the panel structure (i.e., multiple observations for each plant). Alternatively, we could take account of the panel structure of the data by allowing the $\alpha_i$ to differ, and estimate them along with the other parameters. Under these assumptions, we could estimate the model by adding dummy variables for each plant and otherwise following the same procedure as before.

Figure 1 illustrates the potential discrepancy between these assumptions. Consider data from two plants with six observations of total costs and regulatory expenditures for each. As drawn, there is a clear positive effect of rising regulation on total costs for both plant 1 (denoted by $\circ$) and plant 2 (denoted by $\times$). If we view each plant separately, it would appear that there is a roughly \$0.50 increase in total costs for every \$1 increase in regulation. This is the fixed-effects estimate – allowing for different intercepts but forcing the slope to be the same yields a marginal economic cost of environmental expenditure estimate of 0.5.

However, plant 1 has, on average, \$0.50 more regulation than plant 2 and, on average, \$1 more total costs – an increase of \$2 in total costs per dollar of regulation. Pooling the data, in effect averaging the half-for-one fixed-effects relation with this two-for-one relation, we estimate a pooled slope coefficient of 1.5.

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 (e.g., 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.[19]

When there is a discrepancy, as illustrated in Figure 1, it is not immediately obvious which of the two slope estimates – the fixed-effects or the pooled – is "right." If we believe that the differences between plants are actually *caused* by differences in regulation, then something like the pooled estimator may be appealing. 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 plant location. This would be correspond to the pooled estimate, where variation between plants – like 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 characteristic to change, as a *long run* elasticity and the fixed-effects estimate, which holds constant differences between plants, as a *short run* elasticity.[20]

There are three reasons why the above scenario is inappropriate and why we instead prefer the fixed-effects model. First, it seems that there are many more plant characteristics which are likely

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

[20] 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.

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.[21] 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 remains unbiased.

The second reason for preferring the fixed-effects model is actually illustrated by Figure 1. That figure illustrates what we find empirically in Section 4, i.e., that the pooled estimates are uniformly *larger* than the fixed-effects estimates. 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. 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 parameter $\alpha_i$ 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. 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 incongruency with theory, again recommending the fixed-effects approach.[22]

[21] 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.

[22] There are potentially two more econometric reasons one might consider the random-effects model in favor of the fixed-effect model – measurement error and endogeneity. We discuss these below.

## 4 Measuring the True Cost of Environmental Expenditures

To determine the relationship between environmental expenditures and economic cost we first estimate the cost function developed in the previous section. The measure that concerns us – the connection between reported expenditures and economic cost – is then derived based on the estimated parameters. As we will see, this measure differs from plant to plant. In order to compute industry and overall averages, we develop and apply a simple aggregation scheme. This overall average allows us to assess the aggregate relationship between reported environmental expenditures and economic cost.

### 4.1 Cost Function Estimation

We first estimate the cost function and share equations given in (3) using maximum likelihood. The resulting parameter estimates are reported in Table A.2 and described in the Appendix. It is important to note that many of the interaction parameters associated with regulation are statistically significant (e.g., $\beta_{kr}, \beta_{lr}, ...$). For example, the energy share elasticity with respect to regulation $\beta_{er}$ is positive for all eleven industries and statistically significant in eight of eleven, indicating that increases in regulatory expenditure lead to increases in energy demand relative to other factors of production. Higher energy prices therefore raise the cost of additional regulatory expenditure.[23] These kinds of results suggest that our flexible modeling approach, which allows the effect of regulation to vary according to these different interactions, is capturing significant features of the data. Conversely, alternative approaches which assume a simpler relation between regulation and total costs may be misspecified.

Since our goal is to use the estimated parameters to compute the true marginal cost of environmental expenditures, the validity of these parameter estimates is critical.[24] Our estimates hinge on the following key assumptions:

[23]Recall that regulatory expenditure in the model is measured in *real* terms. Higher energy prices may or may not raise the cost of additional *nominal* regulatory expenditure since nominal increments in regulatory expenditure might also rise more in the presence of higher energy prices.

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

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

2. Differences in production technology between plants are fully captured by the fixed effects and do not affect cost shares.

3. The data is measured accurately.

4. Input prices, the level of output, and the level of reported environmental expenditures are fixed prior to the plant's non-environmental production decisions each period.

There are, of course, grounds to question each of these assumptions. The first two assumptions deal 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 somewhat empty from a practical perspective since our model is more flexible than other models which have been applied to this problem. Further, the possibility of additional flexibility is hampered by the amount of data available. For the smaller industries discussed in Section 4.4, one might argue that our specification is *too* flexible.

The third assumption raises the issue of measurement error in the data – a problem widely acknowledged with regard to environmental expenditures. On the one hand, this would tend to bias our estimates downward *if the estimated marginal economic costs of additional environmental expenditures were all positive*.[25] As discussed in Sections 4.3, this is not the case. Since some industries have negative marginal costs and others positive, measurement error could, in fact, lead one to overstate the aggregate marginal cost. On the other hand, measurement may not be as big a problem once we have controlled for fixed plant effects. If the biggest source of measurement error is variation in the way different plants measure environmental expenditures, the fixed-effects estimator will remove this error.[26] Finally, much like the issue of misspecification, there is little

[25]Measurement error in a regressor biases that parameter estimate towards zero. See Section 9.5 of Greene (1990).

[26]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

practical alternative to the proposed approach since there is no alternative measure of environmental expenditures.

The last assumption deals with the issue of endogeneity. Specifically, we are interested in the effect of environmental expenditures on non-environmental expenditures. If non-environmental expenditure instead affect environmental expenditures, or if some omitted variable affects both, the estimated parameters will be biased. For example, if production downtime is a significant expense, plants may have a number of planned production upgrades which remain unimplemented because of the 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 projects makes both environmental and non-environmental expenditures consequences of an omitted variable describing the decision to stop production. Alternatively, Deily and Gray (1991) have argued that the level of regulatory stringency and enforcement may be sensitive to productivity shocks, making environmental expenditures responsive to shocks to non-environmental expenditures.

There is no easy way to deal with this criticism. On the one hand, there has been considerable exogenous variation over time in regulatory stringency.[27] 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.[28] However, 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 problems.[29] As we will see below in Section 4.3, this omitted-variable bias is potentially quite large.

is preserved. We see no reason, however, to believe that measurement error is less related to plant differences than the true variation.

[27]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).

[28]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.

[29]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.

### 4.2 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 how much of *existing* reported expenditures actually reflect *existing* economic costs. Unfortunately, this is a much more complex question because it requires us to determine the entire 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. Such an extrapolation, we believe, would not be credible. We believe, however, that the estimated marginal cost provides some indication of the average cost relation. A marginal cost considerably higher or lower than one would suggest a similar directional for the average cost relation.

To calculate marginal cost, we differentiate the cost function in Equation (2) with respect to *nominal* regulatory expenditures $P_{r,t}R_{i,t}$.[30] This yields an estimate of marginal cost $MC_{i,t}$,

$$\frac{\partial TC_{i,t}}{\partial(P_{r,t}R_{i,t})} = MC_{i,t} = \frac{TC_{i,t}}{P_{r,t}R_{i,t}}(\alpha_r + \beta_r' X_{i,t}) \tag{4}$$

where $TC_{i,t}$ is total cost, $P_{r,t}$ is a price index for regulatory expenditures, $R_{i,t}$ is real regulatory expenditures, $X_{i,t} = \{\log(Y_{i,t}), \log(R_{i,t}), \log(P_{k,i,t}), \log(P_{l,i,t}), \log(P_{e,i,t}), \log(P_{m,i,t}), t\}'$, the vector of output quantities, prices and time as described in Section 3.2, and $\alpha_r$ and $\beta_r$ are parameter estimates from the cost function in Equation (2). Intuitively, this measure reveals the degree to which additional environmental expenditures reflect true economic costs. If this derivative is near unity, increases in reported expenditures are, in fact, a good measure of the additional economic burden of further regulation. On the other hand, if the derivative is not equal to one, such expenditures misrepresent costs, with values greater than one indicating an understatement of true costs and values less than one an overstatement. A value less than zero indicates that increases in regulation

[30] While it more sensible to describe a production function in terms of *real* regulatory expenditures, reflecting abatement effort, the marginal cost is more usefully expressed in terms of *nominal* expenditures. Otherwise, we would be asking how total cost, a nominal measure, changes in response to real environmental expenditures, a real measure. This relationship would depend on the overall price level. The connection between total cost and nominal environmental expenditures, a ratio of two nominal measures, is independent of the overall price level.

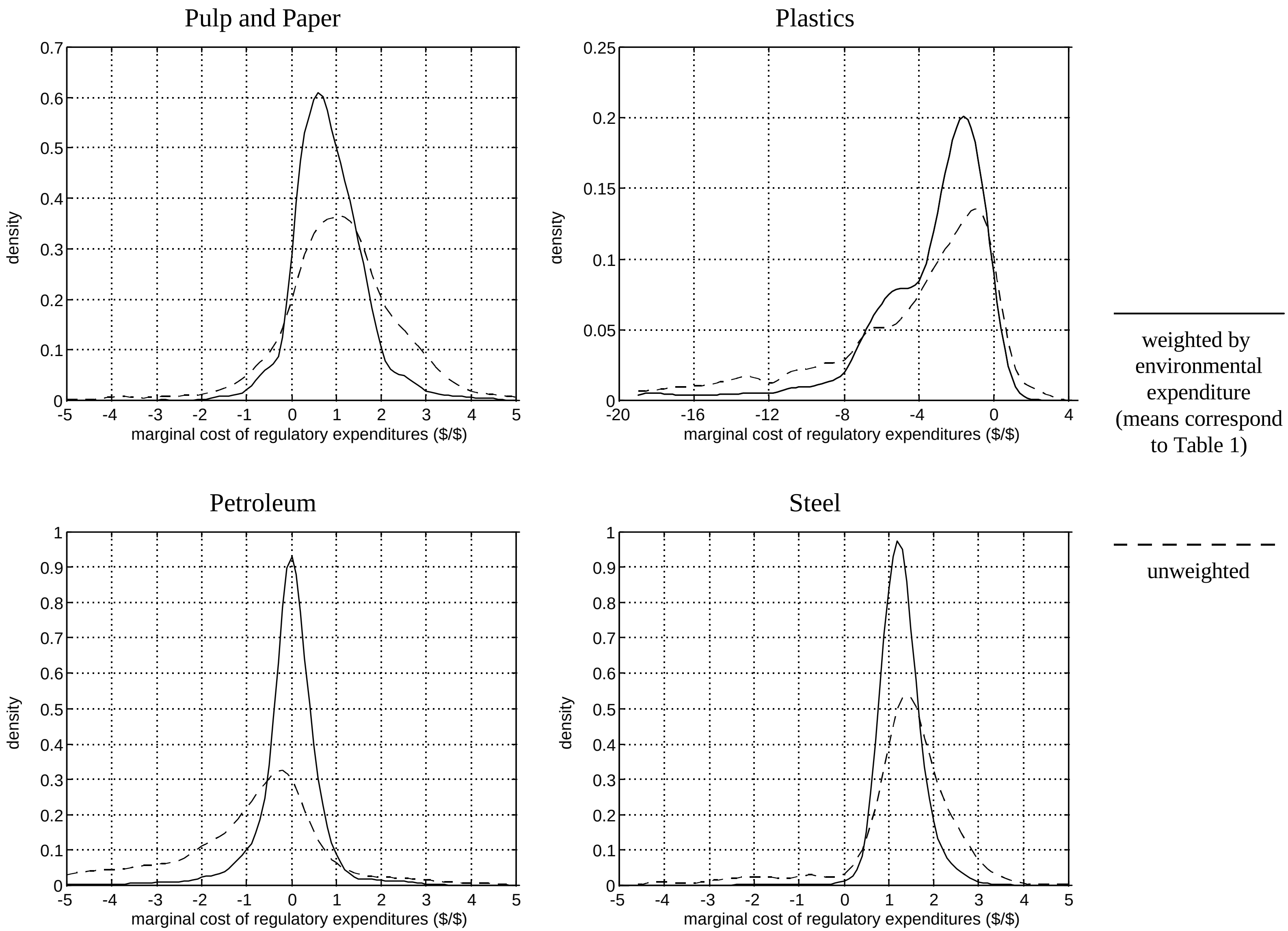
are associated with decreases in total costs, e.g., an economic *benefit*.

Since the value of $MC_{i,t}$ computed in (4) depends on observation specific values of $TC_{i,t}$, $P_{r,t}$, $R_{i,t}$ and $X_{i,t}$, these marginal cost measures will vary from observation to observation. Figure 2 shows, with dashed lines, the distribution of computed marginal costs among individual observations for each of the four large-expenditure industries. In the petroleum refining industry, for example, some plants have estimated marginal costs as high $5 for every dollar of reported environmental expenditures, while others apparently gain – also by as much as $5 – for every dollar of reported expenditures. While these apparent gains may reflect induced innovations of the type described by Porter, they may also represent overreporting of environmental expenditures or the possible accelerating or harvesting of capital improvements that became attractive as a result of the mandated environmental changes.

Figure 2 by itself points to an interesting observation: regardless of any aggregate conclusion about the true economic cost of regulation, there are bound to be both winners and losers. Since every plant reports positive environmental expenditures, the winners may not, in fact, perceive the gains they have realized or at least connect them with environmental regulation. The losers, on the other hand, clearly know they are losing but may not even realize the full extent to which other costs are linked to environmental regulation. This helps explain why many firms claim that costs are higher than reported expenditures even if we find that, on average, they appear to be less.

Beyond the distribution of marginal costs given in Figure 2, we are also interested in average results for different industries as well as the manufacturing sector as a whole. Without such an estimate, it is difficult to know whether, overall, the cost of environmental protection is over- or understated. Therefore, we need to aggregate the distributions in Figure 2. Conceptually, we are deciding how a industry or manufacturing sector change in regulation would likely be allocated among firms in the sample. We could compute a simple arithmetic average over all the values in Figure 2. However, this would amount to dividing up additional expenditures evenly among all observations – even though some plants have much lower regulatory expenditures than others. A more plausible alternative would be to consider the aggregate marginal cost of raising environ-

Figure 2: Distribution of Plant-Level Marginal Costs for Large Expenditure Industries



mental expenditures across plants in proportion to each plant's current expenditures. That is, plants with small expenditures would have small increases and plants with large expenditures would have large increases. Such a scheme involves computing a weighted average where the weights correspond to the level of each observation's nominal regulatory expenditure:[31]

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

In Figure 2, solid lines show the distribution of marginal costs once these weights are applied.[32]

### 4.3 Industry and Manufacturing Sector Estimates

Tables 1 and 2 present aggregate marginal cost estimates based on Equation (5), including multi-industry aggregates in the last column(s). Table 1 focuses on four industries with the largest share of regulatory expenditures – nearly 90% in our sample. These four industries also yield the most stable marginal cost 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 shows results allowing for plant-level, fixed productivity effects. There is considerable variation even among these four, relatively well-behaved industries. Plastics reveals a \$4 cost *savings* for a dollar of increased PACE expenditure, while steel indicates a roughly one-for-one change in total costs for a given change in regulatory expenditures. Petroleum, which has the largest share of reported regulatory expenditures, shows essentially no real cost associated with increases in PACE expenditures. Thus, the observation made in the previous section, that there are both winners and losers when regulation increases, continues to hold at the industry level as well as the individual plant level.

In order to further aggregate the four large expenditure industry results into a single estimate, we can again apply Equation (5). Now, however, we use estimates of environmental spending by

[31]Weighting by real expenditures does not make any difference.
[32]The means of these weighted distributions precisely correspond to the first row of estimates in Table 1.

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

| | Industry: | pulp and paper | plastics | petroleum | steel | cross-industry average |
|---|---|---|---|---|---|---|
| | Full sample | | | | | |
| | # of obs. | 612 | 403 | 708 | 527 | |
| ① | fixed effect | 0.82 | –4.19$^{\dagger *}$ | –0.06 | 1.16 | 0.13 |
| | | (0.43) | (1.04) | (1.12) | (0.71) | (0.61) |
| ② | random effect | 1.18$^{*}$ | 0.21 | 5.06$^{\dagger *}$ | 3.43$^{\dagger *}$ | 3.70$^{\dagger *}$ |
| | | (0.42) | (0.90) | (1.15) | (0.77) | (0.63) |
| | Sample without low expenditure observations[a] | | | | | |
| | # of obs. | 608 | 392 | 666 | 499 | |
| ③ | fixed effect | 0.62 | -6.47$^{\dagger *}$ | 1.09 | 0.71 | 0.39 |
| | | (0.47) | (1.19) | (1.20) | (0.75) | (0.65) |
| | weight[b] | 0.129 | 0.061 | 0.447 | 0.252 | 0.888 |

[a] Observations with nominal environmental expenditures which are less than 1/1000 of the highest observed expenditure are dropped.

[b] Ratio of expenditures in each industry to eleven-industry total. This weight is used to compute cross-industry averages.

$^{*}$Significantly different from zero at the 5% level.

$^{\dagger}$Significantly different from one at the 5% level.

industry to weight each industry's marginal cost estimate.[33] This approach yields an aggregate estimate of only a thirteen cent rise in total costs for every dollar of increased reported regulatory expenditures, with a standard error of sixty-one cents.[34] The standard error of the estimate is quite large, with a 95% confidence interval covering the range $-\$1$ to $+\$1.25$. This does, however, indicate that extremely large values, e.g., $MC \gg \$1$, are unlikely.

The second row of estimates in Table 1 is instead based on a random-effects model. Rather than estimating plant productivity differences as we do in the fixed-effects model, we assume that such differences are completely explained by the included right-hand side variables along with a random disturbance. This means that the effect of any omitted variables – such as location, management style or age – will be attributed to the included right-hand side variables. This confounding of different effects potentially biases the marginal cost estimates. Interestingly, the random-effects estimates are higher than the fixed-effects estimates for all four industries, significantly so in all industries except pulp and paper. The average marginal cost, driven heavily by large increases in the petroleum and steel industries, rises to $3.70 on the dollar.

The random-effects estimate is not only much higher, it is also in line with previous estimates concerning the cost of regulation. Joshi et al. (1997) reports an estimate of $7-12 for the steel industry using a similar cost function modeling approach. Based on a growth accounting approach, 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 which, like our fixed-effects results, are uniformly lower.[35]

[33] We compute environmental expenditure per value of shipments for each industry using our sample, then multiply this ratio by the total value of each industry's shipments in 1988 (based on aggregate Census of Manufactures data) to obtain an estimate of overall regulatory spending. This attempts to capture the sample-based perspective of our study by not using aggregate regulatory expenditures for any one year. We do, however, need to scale our sample upwards based on aggregate data since some industries were more heavily sampled than others (due to the concentration of firms).

[34] Note that with a weight of $0.447/0.888$, petroleum plays an important role in determining the aggregate estimate.

[35] $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 4.1.

The implication of this comparison between the fixed- and random-effect models is striking. Comparing differences *among* plants based on the random-effects model, there appear to be additional costs associated with environmental protection not included in PACE. These additional costs generate a more than dollar-for-dollar increase in total costs for any change in reported environmental expenditures. Going back to Figure 1, this is analogously reflected by the pooled regression line which exhibits a slope greater than one.[36] 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 total costs for that plant, we find that the increase appears to be less than dollar-for-dollar. This is analogous to the fixed-effects regression line in Figure 1 which has a slope less than one. 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 may *overstate* the cost of environmental regulation.

The third and final row of estimates in Table 1 shows the results of a sensitivity analysis where we drop observations with extremely low reported expenditures from our econometric estimation as well as from the calculation of aggregate marginal costs. We motivate this experiment by noting that these observations account for at most two-tenths of one percent of total regulatory expenditures[37] and are thus irrelevant for the computation of aggregate averages. However, including them at the estimation phase puts additional demands on the functional form, which is best viewed as a local approximation, and decreases our confidence in the parameter estimates. In particular, the cost function is forced to capture the behavior of observations with both small and large levels of regulation, even though these firms might behave very differently. Removing observations with extremely low reported expenditures allows us to better estimate the cost function over the relevant range.

[36]Recall that the only difference between pooled and random-effects models is the assumption about the error structure; both assume there are no omitted variables describing plant differences.

[37]The actual selection criterion was whether an observation's level of regulatory expenditure was at least 1/1000 of the largest level observed in the industry.

Table 2: Marginal Cost 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 | 257 | 224 | 80 | 203 | 102 | | |
| ① fixed effect | $5.89^{*}$ | $11.02^{\dagger *}$ | 2.92 | $-7.78^{\dagger}$ | 3.16 | –7.84 | $-57.18^{\dagger *}$ | –3.48 | –0.27 |
| | (2.76) | (4.66) | (2.10) | (4.09) | (4.38) | (9.21) | (19.63) | (4.01) | (0.70) |
| ② random effect | 1.46 | $13.82^{\dagger *}$ | $8.21^{\dagger *}$ | 6.86 | $14.69^{\dagger *}$ | 13.36 | –12.16 | $10.49^{\dagger *}$ | $4.46^{\dagger *}$ |
| | (2.40) | (4.91) | (2.51) | (3.91) | (5.62) | (8.89) | (18.19) | (3.93) | (0.71) |
| Sample without low expenditure observations[a] | | | | | | | | | |
| # of obs. | 182 | 114 | 232 | 195 | 78 | 200 | 98 | | |
| ③ fixed effect | 3.27 | $11.02^{\dagger *}$ | 3.03 | $-11.78^{\dagger *}$ | 4.44 | –12.98 | $-47.37^{\dagger *}$ | –5.62 | –0.28 |
| | (2.89) | (4.66) | (2.27) | (4.66) | (4.62) | (10.13) | (19.71) | (4.39) | (0.76) |
| weight[b] | 0.010 | 0.007 | 0.016 | 0.008 | 0.020 | 0.047 | 0.004 | 0.112 | |

[a] Observations with nominal environmental expenditures which are less than 1/1000 of the highest observed expenditure are dropped from both estimation and marginal cost calculation.
[b] Ratio of expenditures in each industry to eleven-industry total. This weight is used to compute cross-industry averages.
$^{*}$Significantly different from zero at the 5% level.
$^{\dagger}$Significantly different from one at the 5% level.

These results show some change relative to the full sample estimates, but not a statistically significant one. Only the plastics industry experiences a statistically significant change, but its small share in total expenditures means that the average is not significantly affected.[38] The overall average in this case is thirty-nine cents on the dollar – slightly higher, but still in the zero to one range and still quite noisy.

### 4.4 Small Expenditure Industries

We summarize the results for small expenditure industries in Table 2. No single industry in this group accounts for more than 5% of total regulatory expenditures in the eleven industries and, as a group, they account for only 11%. The estimated marginal costs for individual industries, which range from -57 to 11, exceed any plausible range one might imagine to be accurate. Simultaneously, the standard errors are much higher – an average of $4 versus $0.60 for the large expenditure industries.

On the one hand, these industries all have much smaller sample sizes compared to those in Table 1. On the other hand, they include many industries like semiconductors and pharmaceuticals which may not yield to traditional production function modeling (plastics might also fall in this category). Specifically, the assumption of a single, fairly homogenous output is challenged by the variety of products. Individual plants may be more differentiated than their common four-digit SIC code suggests. Both of these issues make our cost function approach somewhat questionable when applied to these industries.

Another important distinction is between industries with primarily end-of-pipe expenditures versus those which rely more on process changes. End-of-pipe expenditures, wherein 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 estimate the level of environmental expenditures with versus without regulation. As the menu of manufacturing technologies changes in response to demand for cleaner processes, with the dirtier alternatives being eliminated, the difficulty of estimating environmental relative to an absence-of-regulation baseline may increase. Since most of the small expenditure industries are process-change oriented, it is not surprising that the corresponding estimates of total cost/PACE expenditure relations are more exotic.

While it would have been reassuring to see results in the small expenditure industries parallel

[38] Plastics, along with petroleum, is an industry where process changes are more likely than end-of-pipe treatment. As discussed in the next section, this makes the relation between reported expenditures and true costs more difficult to predict.

those in the large expenditure industries, we find two useful messages in Table 2. First, some patterns remain: the random-effects estimates remain higher than the fixed-effects estimates for all but one of the small expenditure industries (malt beverages). Second, the wide-ranging and implausible estimates may simply reflect the poor quality of the underlying PACE data. That is, there may not be a systematic relationship between reported environmental expenditures and economic costs in some industries.

## 5 Conclusion

Most previous analyses find that reported environmental expenditures are likely to *understate* the true economic cost of environmental protection. Surprisingly, our results suggest that the cost of environmental protection may be *overstated*. Our preferred calculations imply that a dollar of additional regulatory expenditure is associated with only thirteen cents in higher total costs.[39] A ninety-five percent confidence interval for the true answer, however, ranges from a dollar of savings to $1.25 in higher costs. This includes the possibility that total costs rise dollar-for-dollar with reported expenditures but indicates that multipliers much higher than one are unlikely. It puts considerable weight on the possibility that the true costs are less than reported compliance expenditures.

An important finding in our work is that alternative assumptions about productivity differences among plants produce vastly different results. Use of a random-effects model generates an aggregate estimate of $3.70 in higher costs for every additional dollar of reported regulatory expenditure – versus $0.13 for the fixed-effects model. In contrast to the fixed-effects specification, the random-effects specification assumes that unmodeled differences between plants (e.g., age, location, management style) are unrelated to either total costs or reported environmental expenditures. We believe that this assumption is implausible and that the random-effects estimates are therefore biased. This is why our analysis focuses primarily on the fixed-effects model.

We also observe a large amount of variation in our estimates both within and among industries.

[39] Using the same method but a slightly reduced sample raises the estimate to thirty-nine cents.

This observation is facilitated by our use of a cost-function modeling approach and by our consideration of eleven different manufacturing industries. While challenging our attempts to summarize and explain the relationships between reported expenditures and economic costs, the wide variation is itself an interesting result. In particular, such variation may help explain why many firms believe that PACE understates the true cost of environmental protection even if, on average, PACE is roughly right or even overstates true costs.

Any explanation for the full range of our estimated results must cover not only under- and overstated costs, but also how some plants and even entire industries could see total costs fall as reported environmental expenditures rise. Explanations for understated costs abound: reduced flexibility, crowding out of new investments, new source bias, or simply poor accounting. The list for overstated costs is shorter: offsets or, once again, poor accounting (perhaps intentional). Offsets represent pecuniary benefits associated with environmental protection such as recyclable effluents or savings in other areas of production.

Regarding the potential for a negative relation between reported expenditures and total costs, only two explanations seem plausible. The first is that some plants simply operate sub-optimally and regulation, in fact, improves their overall efficiency by inducing innovation. While a plausible explanation for the behavior of some individual plants, this would seem less plausible for an entire industry, such as plastics. A second possibility arises when ordinary investment projects are correlated with environmental expenditures. This could occur, for example, if there is a high cost associated with shutting down a production line. Once the line is shut down for environmental improvements, plant managers might simultaneously undertake non-environmental investments to take advantage of the forced downtime. Alternatively, plants could be including the costs of ordinary investments – either inadvertently or not – in reported environmental expenditures. If these non-environmental projects subsequently lower total costs, this will generate a negative correlation between total costs and environmental expenditures. It is unclear if this is a true consequence of environmental spending or a source of bias.

Whether estimates of current expenditures on environmental protection are, in the aggregate, over- or understated remains open. While further research is needed, these results suggest that the possibility of overstatement is quite real. As for future environmental policies, it is fair to say that the current emphasis on better measurement of benefits ought to be balanced with greater attention to uncertainties about costs. In particular, cost analyses should give greater consideration to possible pecuniary benefits and induced innovations associated with environmental expenditures.

## References


Ashford, N. A., C. Ayers, and R. Stone (1985). Using regulation to change the market for innovation. *Harvard Environmental Law Review 9,* 419–466.

Bailey, E. E. and A. F. Friedlaender (1982). Market structure and multiproduct industries. *Journal of Economic Literature 20*, 1024–1048.

Barbera, A. J. and V. D. McConnell (1990). The impact of environmental regulation on industry productivity: Direct and indirect effects. *Journal of Environmental Economics and Management 18,* 50–65.

Barrett, S. (1992). Stategy and environment. *Columbia Journal World Business 27*, 202–208.

Bartelsman, E. J. and W. B. Gray (1994). *NBER Productivity Database* [online]. URL:ftp://nber.nber.org/pub/productivity.

Bartik, T. J. (1988). The effects of environmental regulation on business location in the United States. *Growth Change 19*(3), 22–44.

Bartik, T. J. (1989). Small business start-ups in the United States: Estimates of the effects of characteristics of states. *Southern Economic Journal 55*(4), 1004–1018.

Berndt, E. R. (1990). *The Practice of Econometrics: Classic and Contemporary*. New York: Addison-Wesley.

Caves, D. W., L. R. Christensen, M. W. Tretheway, and R. J. Windle (1985). Network effects and the measurement of returns to scale and density for U.S. railroads. In A. F. Daughety (Ed.), *Analytical Studies in Transport Economics,* pp. 97–120. Cambridge University Press.

Christensen, L. and D. Jorgenson (1969). The measurement of U.S. real capital input, 1929-1967. *Review of Income and Wealth*, 293–320.

Deily, M. E. and W. B. Gray (1991). Enforcement of pollution regulation in a declining industry. *Journal of Environmental Economics and Management 21*, 260–274.

Diewert, W. and T. Wales (1987). Flexible functional forms and global curvature conditions. *Econometrica 55*(1), 43–68.

Friedman, J., D. A. Gerlowski, and J. Silberman (1992). What attracts foreign multinational corporations? evidence from branch plant location in the United States. *Journal of Regional Science 32*(4), 403–418.

Gray, W. B. (1987). The cost of regulation: OSHA, EPA and the productivity slowdown. *American Economic Review 77*(5), 998–1006.

Gray, W. B. and R. J. Shadbegian (1994). Pollution abatement costs, regulation and plant-level productivity. Discussion Paper, U.S. Department of Commerce, Center for Economic Studies.

Greene, W. H. (1990). *Econometric Analysis*. New York: MacMillan.

Gruenspecht, H. K. (1982). Differentiated regulation: the case of auto emission standards. *American Economic Review 72*, 328–331.

Hall, R. and D. Jorgenson (1967). Tax policy and investment behavior. *American Economic Review 57*, 391–414.

Hausman, J. (1982). Specification tests in econometrics. *Econometrica 46*(6), 1251–1271.

Hazilla, M. and R. J. Kopp (1990). Social cost of environmental quality regulation: A general equilibrium analysis. *Journal of Political Economy 98*(4), 853–873.

Hulten, C. R. and F. C. Wykoff (1981). The measurement of economic depreciation. In C. R. Hulten (Ed.), *Depreciation, Inflation, and the Taxation of Income from Capital*. Washington: Urban Institute Press.

Jaffee, A. B., S. R. Peterson, P. R. Portney, and R. N. Stavins (1995). Environmental regulation and the competitiveness of U.S. manufacturing: What does the evidence tell us? *Journal of Economic Literature 33*, 132–163.

Jorgenson, D. and R. Landau (1993). *Tax Reform and the Cost of Capital: An International Comparison*. Washington: Brookings.

Jorgenson, D. and K.-Y. Yun (1991). *Tax Reform and the Cost of Capital*. Oxford: Claredon Press.

Jorgenson, D. W. and P. J. Wilcoxen (1990). Environmental regulation and U.S. economic growth. *Rand Journal of Economics 21*(2), 314–340.

Joshi, S., L. Lave, J.-S. Shih, and F. McMichael (1997). Impact of environmental regulations on the U.S. steel industry. Mimeo, Carnegie Mellon University.

Leibenstein, H. (1966). Allocative efficiency vs. 'X-efficiency'. *American Economic Review 56*, 392–415.

Levinson, A. (1992). Environmental regulation and manufacturers' location choices: Evidence from the census of manufactures. New York: Columbia University.

McConnell, V. D. and R. M. Schwab (1990). The impact of environmental regulation on industry location decisions: The motor vehicle industry. *Land Economics 17*(3), 67–81.

Mundlak, Y. (1978). On the pooling of time series and cross section data. *Econometrica 46*(1), 69–85.

Nelson, R. A., T. Tietenberg, and M. R. Donihue (1993). Differential environmental regulation: Effects on electric utility capital turnover and emissions. *Review of Economics and Statistics 77*(2), 368–373.

Palmer, K. L., W. E. Oates, and P. R. Portney (1995). Tightening environmental standards: The benefit-cost or the no-cost paradigm? *Journal of Economic Perspectives 9*(4), 119–132.

Palmer, K. L. and D. R. Simpson (1993). Environmental policy as industrial policy. *Resources 10*(4), 371–382.

Porter, M. E. (1991, April). America's green strategy. *Scientific American*.

Porter, M. E. and C. Van der Linde (1995). Toward a new conception of the environment-competitiveness relationship. *Journal of Economic Perspectives 9*(4).

Rose, A. (1983). Modeling the macroeconomic impact of air pollution abatement. *Journal of Regulatory Science 23*(4), 441–459.

Schmalensee, R. (1993). The costs of environmental protection. In *Balancing Economic Growth and Environmental Goods*, pp. 53–80. Washington, DC: American Council for Capital Formation.

Simpson, D. R. and R. L. Bradford (1996). Taxing variable cost: Environmental regulation as industrial policy. *Journal of Environmental Economics and Management 30*(3), 282–300.

Streitweiser, M. L. (1996). Evaluation and use of the pollution abatement costs and expenditure survey. Mimeo, Center for Economic Studies, Bureau of Census.

Ulph, A. (1994). Environmental policy and international trade: A survey of recent economic analysis. Milan, Italy: Nota de Lavoro 53.94, Fondazione Eni Enrico Matti.

U.S. Department of Commerce (various issues). *Survey of Current Business*.

U.S. Environmental Protection Agency (1990). Environmental investments: The cost of a clean environment.

U.S. Office of Technology Assessment (1980). Technology and steel industry competitiveness. OTA-M-122, Washington, DC.

U.S. Office of Technology Assessment (1994). Industry, technology and the environment: Competitiveness, challenges, and business opportunities. OTA-ITE-586, Washington, DC.

Varian, H. R. (1992). *Microeconomic Analysis* (third ed.). New York: Norton.

# A Appendix – Data Description and Detailed Estimation Results

## A.1 Data Sources

The data used in this paper are drawn from from several large 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 their 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. The detailed quantity and expenditure information for energy consumption is only available up to 1981.

Table A.1: Sample Size by Industry

| Industry | No. of Plant | No. of Sample |
|---|---|---|
| Malt Beverage | 45 | 185 |
| Pulp and Paper | 100 | 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 |

- *The Manufacturing Energy Consumption Surveys (MECS)*. This survey is collected by the Department of Energy every three years beginning 1985. It 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. The sample size of final available dataset for each of the eleven industries is given in Table A.1.

Data on input and output quantities and prices is constructed according to following procedures.

- *Output.* Data on the total value of shipments, by individual product codes, are contained in the LRD. We construct a output price index by summing the producer price index of different products obtained from Bureau of Labor Statistics, weighted by the shares of individual product:

$$p_{agg} = \sum_i PPI_i \times \frac{VS_i}{\sum_j VS_j} \tag{A.1}$$

  where $p_{agg}$ is the aggregated output price index, $PPI_i$ is the produce price index of different outputs produced by the plant, and $VS_i$ is the value share of each of the different outputs in the total value of shipments. 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.

- *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.[40] 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:

$$k_t = (1-\delta)k_{t-1} + \frac{q_0}{q_t} I_t \tag{A.2}$$

  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 ($\delta$) 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:

$$p_{k(t)} = [q_{t-1}r_t + \delta q_t - (q_t - q_{t-1}) + q_t C_t]\frac{1 - u_t z_t - k_t}{1 - u_t} \tag{A.3}$$

  where,

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

  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.

[40] 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.

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

- *Price of Materials.* The data on total expenditures on individual materials are collected only once every five years in the Census of Manufacturers (CM). We derive a single index of the price of materials for each plant by averaging the producer price indices of individual materials weighted by each plants' cost shares for the years 1977, 1982, 1987, 1992. We then linearly interpolate these plant-level material price indices to derive indices for the intervening years.

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

- *Price of Energy.* Detailed data on total quantities consumed and total expenditures on various fuel were collected in LRD (through 1981) and MECS (1985, 88, 91). We use this data to calculate the prices of individual fuels ($/Mbtu) paid by each plant in our sample. The individual fuels considered include coal, natural gas, dfo, rfo, lpg and electricity. These fuels typically account about 90 percent of total energy cost. The price of energy is the average price of individual fuels weighted by cost share.

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

### A.2 Estimation

Table A.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 27 free slope parameters in (2), standard errors, goodness of fit and likelihood statistics are reported. Table A.3 provides additional information about the consistency of the estimated share values, fitted share values and own-price demand elasticities.

First, note that many of the second-order terms involving reglation ($\beta_{kr}, \beta_{lr}, \beta_{er},$ etc.) are significant. This confirms our original motivation for using a flexible cost function: the effect of regulation clearly varies from observation to observation based on these second order effects. A model which does not allow for these second-order effects would be misspecified.

The goodness of fit statistics and the tests against more restrictive models are also informative. The cost functions are generally fit better ($R^2 > 0.93$) than the share equations ($R^2 < 0.77$). This

Table A.2: Estimation results
(standard errors are in parentheses)

| Sector: | malt beverages | pulp and paper | printing | plastics | pharmaceuticals | petroleum | steel | refrigeration | semiconductors | motor vehicles | aircraft engines |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $\alpha_k$ | 0.0655 | 0.1145 | 0.0878 | 0.0824 | 0.1160 | 0.0269 | 0.0783 | 0.0401 | 0.2390 | 0.0203 | 0.0691 |
| | (0.0026) | (0.0024) | (0.0050) | (0.0022) | (0.0046) | (0.0009) | (0.0020) | (0.0019) | (0.0110) | (0.0010) | (0.0043) |
| $\alpha_l$ | 0.1345 | 0.1991 | 0.3439 | 0.0885 | 0.2406 | 0.0191 | 0.2147 | 0.2437 | 0.3041 | 0.1105 | 0.2739 |
| | (0.0038) | (0.0024) | (0.0112) | (0.0028) | (0.0093) | (0.0006) | (0.0048) | (0.0072) | (0.0201) | (0.0035) | (0.0152) |
| $\alpha_e$ | 0.0272 | 0.1170 | 0.0261 | 0.0697 | 0.0436 | 0.0240 | 0.0961 | 0.0172 | 0.0520 | 0.0058 | 0.0209 |
| | (0.0008) | (0.0025) | (0.0014) | (0.0048) | (0.0028) | (0.0010) | (0.0033) | (0.0009) | (0.0037) | (0.0002) | (0.0015) |
| $\alpha_y$ | 0.7527 | 0.6338 | 0.7585 | 0.7305 | 0.3749 | 0.7493 | 0.5869 | 0.6336 | 0.4480 | 0.7584 | 0.7810 |
| | (0.0321) | (0.0271) | (0.0651) | (0.0331) | (0.0429) | (0.0343) | (0.0282) | (0.0290) | (0.0828) | (0.0285) | (0.0428) |
| $\alpha_r$ | 0.0494 | 0.0275 | 0.0544 | –0.0625 | 0.0165 | –0.0054 | 0.0411 | –0.0104 | 0.0712 | –0.0024 | –0.1126 |
| | (0.0191) | (0.0132) | (0.0218) | (0.0188) | (0.0279) | (0.0147) | (0.0181) | (0.0201) | (0.0488) | (0.0237) | (0.0348) |
| $\alpha_t$ | –0.0093 | –0.0007 | 0.0146 | 0.0050 | 0.0231 | –0.0031 | –0.0124 | 0.0081 | –0.0562 | –0.0091 | 0.0237 |
| | (0.0026) | (0.0017) | (0.0037) | (0.0027) | (0.0045) | (0.0026) | (0.0026) | (0.0041) | (0.0138) | (0.0035) | (0.0083) |
| $\beta_{kk}$ | 0.0791 | 0.0658 | 0.0105 | 0.0354 | 0.0112 | 0.0078 | 0.0592 | 0.0107 | 0.1303 | 0.0123 | 0.0085 |
| | (0.0127) | (0.0176) | (0.0285) | (0.0118) | (0.0127) | (0.0013) | (0.0082) | (0.0042) | (0.0473) | (0.0046) | (0.0173) |
| $\beta_{ll}$ | 0.0225 | 0.0595 | 0.0665 | 0.0175 | 0.0184 | 0.0007 | 0.1515 | 0.0888 | 0.1258 | 0.0159 | 0.0191 |
| | (0.0181) | (0.0091) | (0.0380) | (0.0093) | (0.0201) | (0.0015) | (0.0174) | (0.0149) | (0.0430) | (0.0152) | (0.0403) |
| $\beta_{ee}$ | 0.0019 | 0.0232 | 0.0091 | –0.0186 | 0.0132 | –0.0079 | 0.0404 | 0.0013 | 0.0338 | 0.0011 | 0.0021 |
| | (0.0016) | (0.0057) | (0.0032) | (0.0097) | (0.0030) | (0.0015) | (0.0066) | (0.0014) | (0.0049) | (0.0004) | (0.0028) |
| $\beta_{yy}$ | –0.0715 | –0.0155 | 0.1371 | 0.0279 | –0.0035 | 0.0103 | 0.1017 | 0.0707 | 0.2278 | 0.2595 | 0.1262 |
| | (0.0461) | (0.0355) | (0.1102) | (0.0358) | (0.0579) | (0.0259) | (0.0270) | (0.0337) | (0.0735) | (0.0324) | (0.0209) |
| $\beta_{rr}$ | 0.0202 | 0.0142 | 0.0113 | 0.0095 | 0.0001 | –0.0034 | 0.0278 | 0.0241 | 0.0066 | 0.0710 | 0.0467 |
| | (0.0096) | (0.0064) | (0.0148) | (0.0086) | (0.0131) | (0.0071) | (0.0098) | (0.0094) | (0.0217) | (0.0236) | (0.0265) |
| $\beta_{tt}$ | –0.0007 | 0.0027 | –0.0025 | –0.0025 | 0.0006 | –0.0020 | 0.0005 | –0.0003 | –0.0020 | –0.0030 | –0.0062 |
| | (0.0010) | (0.0006) | (0.0014) | (0.0009) | (0.0015) | (0.0009) | (0.0009) | (0.0012) | (0.0030) | (0.0013) | (0.0020) |
| $\beta_{kl}$ | 0.0073 | –0.0007 | –0.0504 | –0.0058 | 0.0137 | 0.0009 | –0.0061 | 0.0044 | –0.0692 | 0.0062 | 0.0000 |
| | (0.0107) | (0.0089) | (0.0157) | (0.0073) | (0.0097) | (0.0007) | (0.0072) | (0.0039) | (0.0235) | (0.0049) | (0.0152) |
| $\beta_{ke}$ | 0.0053 | –0.0148 | –0.0030 | 0.0052 | –0.0005 | 0.0019 | 0.0013 | 0.0039 | 0.0184 | 0.0030 | 0.0064 |
| | (0.0030) | (0.0053) | (0.0066) | (0.0045) | (0.0039) | (0.0010) | (0.0034) | (0.0017) | (0.0091) | (0.0007) | (0.0049) |
| $\beta_{ky}$ | 0.0088 | 0.0165 | –0.0126 | 0.0043 | –0.0059 | –0.0042 | –0.0020 | –0.0034 | 0.0241 | –0.0038 | –0.0122 |
| | (0.0039) | (0.0038) | (0.0071) | (0.0026) | (0.0039) | (0.0011) | (0.0023) | (0.0016) | (0.0086) | (0.0015) | (0.0030) |
| $\beta_{kr}$ | 0.0026 | 0.0118 | 0.0152 | 0.0124 | 0.0096 | 0.0019 | 0.0091 | 0.0048 | –0.0050 | 0.0021 | 0.0090 |
| | (0.0020) | (0.0021) | (0.0038) | (0.0016) | (0.0022) | (0.0007) | (0.0014) | (0.0011) | (0.0061) | (0.0012) | (0.0023) |
| $\beta_{kt}$ | 0.0010 | 0.0046 | 0.0079 | 0.0024 | 0.0087 | 0.0021 | 0.0020 | 0.0020 | 0.0282 | 0.0014 | 0.0074 |
| | (0.0006) | (0.0006) | (0.0013) | (0.0005) | (0.0010) | (0.0002) | (0.0005) | (0.0004) | (0.0026) | (0.0003) | (0.0011) |

Table A.2: Estimation results (continued)
(standard errors are in parentheses)

| Sector: | malt beverages | pulp and paper | printing | plastics | pharmaceuticals | petroleum | steel | refrigeration | semiconductors | motor vehicles | aircraft engines |
|---|---|---|---|---|---|---|---|---|---|---|---|
| $\beta_{le}$ | 0.0101 | –0.0152 | –0.0029 | –0.0163 | –0.0109 | –0.0027 | –0.0273 | 0.0029 | –0.0156 | 0.0008 | –0.0045 |
| | (0.0036) | (0.0039) | (0.0048) | (0.0047) | (0.0052) | (0.0007) | (0.0070) | (0.0017) | (0.0060) | (0.0009) | (0.0052) |
| $\beta_{ly}$ | 0.0004 | –0.0016 | –0.0454 | –0.0247 | –0.0301 | –0.0093 | –0.0204 | –0.0282 | –0.0405 | –0.0265 | –0.0629 |
| | (0.0058) | (0.0040) | (0.0170) | (0.0034) | (0.0085) | (0.0007) | (0.0055) | (0.0064) | (0.0165) | (0.0052) | (0.0105) |
| $\beta_{lr}$ | –0.0073 | 0.0007 | 0.0065 | 0.0188 | 0.0172 | 0.0029 | 0.0073 | 0.0234 | –0.0021 | 0.0129 | 0.0367 |
| | (0.0029) | (0.0022) | (0.0091) | (0.0021) | (0.0046) | (0.0004) | (0.0035) | (0.0042) | (0.0117) | (0.0041) | (0.0087) |
| $\beta_{lt}$ | –0.0013 | –0.0014 | –0.0023 | 0.0002 | –0.0031 | 0.0005 | –0.0069 | –0.0057 | –0.0123 | –0.0023 | –0.0111 |
| | (0.0009) | (0.0006) | (0.0025) | (0.0006) | (0.0018) | (0.0001) | (0.0010) | (0.0014) | (0.0043) | (0.0009) | (0.0033) |
| $\beta_{ey}$ | –0.0082 | –0.0073 | –0.0114 | 0.0148 | –0.0139 | –0.0084 | –0.0108 | –0.0036 | –0.0065 | –0.0023 | –0.0050 |
| | (0.0012) | (0.0041) | (0.0021) | (0.0055) | (0.0026) | (0.0012) | (0.0037) | (0.0007) | (0.0030) | (0.0003) | (0.0010) |
| $\beta_{er}$ | 0.0026 | 0.0145 | 0.0059 | 0.0003 | 0.0126 | 0.0041 | 0.0035 | 0.0027 | 0.0039 | 0.0014 | 0.0040 |
| | (0.0006) | (0.0023) | (0.0011) | (0.0037) | (0.0014) | (0.0007) | (0.0024) | (0.0005) | (0.0022) | (0.0003) | (0.0008) |
| $\beta_{et}$ | –0.0007 | –0.0052 | –0.0003 | 0.0015 | –0.0010 | 0.0005 | –0.0011 | –0.0005 | 0.0013 | –0.0003 | –0.0001 |
| | (0.0002) | (0.0005) | (0.0004) | (0.0009) | (0.0006) | (0.0002) | (0.0006) | (0.0002) | (0.0008) | (0.0001) | (0.0004) |
| $\beta_{yr}$ | –0.0076 | –0.0086 | –0.0149 | –0.0438 | 0.0312 | 0.0133 | –0.0317 | –0.0531 | –0.0262 | –0.1136 | –0.0805 |
| | (0.0176) | (0.0095) | (0.0323) | (0.0110) | (0.0215) | (0.0108) | (0.0113) | (0.0150) | (0.0329) | (0.0288) | (0.0259) |
| $\beta_{yt}$ | –0.0101 | 0.0074 | 0.0002 | 0.0092 | 0.0107 | –0.0026 | 0.0012 | –0.0101 | –0.0279 | –0.0078 | 0.0045 |
| | (0.0033) | (0.0019) | (0.0069) | (0.0026) | (0.0044) | (0.0022) | (0.0023) | (0.0040) | (0.0101) | (0.0042) | (0.0036) |
| $\beta_{rt}$ | 0.0033 | –0.0044 | 0.0051 | –0.0008 | –0.0043 | –0.0004 | –0.0005 | 0.0060 | 0.0116 | –0.0053 | 0.0013 |
| | (0.0016) | (0.0011) | (0.0041) | (0.0017) | (0.0018) | (0.0013) | (0.0015) | (0.0024) | (0.0066) | (0.0032) | (0.0049) |
| observations | 185 | 612 | 114 | 403 | 257 | 708 | 527 | 224 | 80 | 203 | 102 |
| plants | 45 | 142 | 45 | 107 | 73 | 165 | 127 | 76 | 28 | 59 | 29 |
| $R^2$ total cost | 0.98 | 0.95 | 0.99 | 0.96 | 0.96 | 0.95 | 0.97 | 0.98 | 0.93 | 0.97 | 0.97 |
| $R^2$ capital share | 0.42 | 0.42 | 0.49 | 0.37 | 0.50 | 0.35 | 0.34 | 0.43 | 0.77 | 0.40 | 0.62 |
| $R^2$ labor share | 0.04 | 0.00 | 0.13 | 0.19 | 0.11 | 0.27 | 0.15 | 0.30 | 0.26 | 0.12 | 0.30 |
| $R^2$ energy share | 0.28 | 0.19 | 0.28 | 0.04 | 0.26 | 0.10 | 0.11 | 0.19 | 0.35 | 0.24 | 0.28 |
| log-likelihood | 1570 | 3565 | 810 | 2260 | 1415 | 6241 | 2829 | 2053 | 463 | 2149 | 797 |
| versus pooled | 1469 | 3168 | 718 | 2039 | 1114 | 5897 | 2539 | 1847 | 357 | 2023 | 691 |
| $\chi^2$: f.e. $\equiv$ r.e. | 52 | 268 | 34 | 193 | 77 | 243 | 274 | 256 | 49 | 135 | 87 |
| d.o.f. | 16 | 21 | 16 | 16 | 17 | 17 | 14 | 18 | 17 | 12 | 16 |

is not surprising: there are many more right-hand side variables in the cost function.[41] Also, it is more difficult to predict factor shares versus total cost since factor demands can vary while total cost is unchanged, but not vice-versa.

The first test against more restrictive models compares the maximized likelihood of the estimated model with that of a simple pooled model. The pooled model assumes the fixed-effects are all the same ($\alpha_i$ in Equation (2)). In every industry, the likelihood ratio test rejects the hypothesis that the pooled model is a valid restriction of the more general fixed-effects model. This result raises questions about previous work (Gray and Shadbegian 1994; Joshi, Lave, Shih, and McMichael 1997) based on the pooled model.

The second, Wald-like statistic compares the parameters of the estimated fixed-effects model with those of an alternative model assuming random effects. In particular, we test the hypothesis that all the (slope) parameter estimates are the same.[42] Like the pooled model, the random-effects model assumes that there are no omitted variables such as location, age and management which might bias the paramater estimates. Unlike the pooled model, however, the random-effects model allows for the fact that the errors for multiple observations from the same plant might be correlated. Again, the resulting test statistics reject in all eleven industries at the 1% level. This indicates that the random-effects model is misspecified in favor of the fixed-effects model.

Table A.3 takes the estimates from Table A.2 and examines their consistency in light of economic theory. We first compare the estimated cost shares to values observed 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 – a likely consequence of 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 demand) are positive and whether

[41] An adjusted $R^2$ cannot be calculated for individual equations since the equations are estimated jointly.

[42] This amounts to a specification test of the random-effects model (see Hausman 1982). That is, the fixed-effects model relaxes the assumption that the plant effects are random and uncorrelated with the other regressors. Unlike Hausman, we allow for covariance between the two estimators in computing the test statistic, even though such correlation would vanish asymptotically.

the own-price elasticities are negative. Only a few of the fitted cost shares turn out to be negative but a large number of the capital own-price elasticities are positive. This means that the factor demand schedule is locally upward sloping, contradicting economic theory. These results are a likely consequence of the difficulty in calculating accurate capital prices (see discussion in Section A.1).

By estimating two alternative models, we verify that the observed positive own-price elasticities for capital do not have important consequences for our primary results. These models restrict the factor demand for capital by either (a) assuming Cobb-Douglas technology (imposing an own-price elasticity of -1); or (b) treating capital as a quasi-fixed input (so only variable cost – labor, energy, materials – is minimized). The Cobb-Douglas specification leads to an aggregate[43] estimate of the marginal economic cost of environmental expenditures of 0.02 while the fixed capital stock model yields an estimate of 0.56. Compared to the 0.13 estimate in Section 4.3, these are insignificant differences (about one-half of the estimated standard error).

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

Table A.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 A.2 estimates | capital | 0.066 | 0.114 | 0.088 | 0.082 | 0.116 | 0.027 | 0.078 | 0.040 | 0.239 | 0.020 | 0.069 |
| | labor | 0.134 | 0.199 | 0.344 | 0.088 | 0.241 | 0.019 | 0.215 | 0.244 | 0.304 | 0.110 | 0.274 |
| | energy | 0.027 | 0.117 | 0.026 | 0.070 | 0.044 | 0.024 | 0.096 | 0.017 | 0.052 | 0.006 | 0.021 |
| | material | 0.773 | 0.569 | 0.542 | 0.759 | 0.600 | 0.930 | 0.611 | 0.699 | 0.405 | 0.863 | 0.636 |
| 1987 I-O tables | 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 | 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) | | | | | | | | | | | | |
| | capital | 0.016 | 0.010 | 0.018 | 0.017 | | 0.047 | 0.008 | 0.009 | | 0.015 | 0.020 |
| | labor | | | | 0.005 | | | | | | | |
| | energy | | | 0.009 | | 0.031 | 0.001 | | | | | |
| | material | | | | | | | | | | | |
| Fraction of observations with positive own-price elasticities (zeros are omitted) | | | | | | | | | | | | |
| | capital | 0.892 | 0.229 | 0.061 | 0.127 | 0.004 | 0.171 | 0.491 | 0.071 | 0.750 | 0.345 | 0.039 |
| | labor | | | | 0.007 | | 0.001 | 0.074 | | | | |
| | energy | | | 0.026 | | 0.160 | | 0.004 | | 0.238 | | |
| | material | 0.027 | | | | | | | 0.004 | | | |