Comments for authors on:
"A Fresh Look at the Benefits and Costs of the U.S. Acid Rain Program."
For *Journal of Environmental Management*

This manuscript reviews the changes in the major environmental and public health endpoints, associates economic values of the benefits and compares them with costs of the acid rain program. I think the assessment is accurate on net and an important update to conventional understanding about Title IV.

The article might be improved if it expanded the literature review somewhat. An article of this length cannot encompass the broad technical literature that is relevant to each of the environmental and public health endpoints that are addressed. However, major assessments of benefits and/or costs should be discussed. At this point only the EPA's own assessment, and Ostro et al.'s assessment, are described. Three others that also could be discussed are:

For the cost side:
Ellerman et al. 2000. *Markets for Clean Air;* and
Carlson et al. 2000. *Journal of Political Economy*.

For the benefit side (integrated assessment):
Burtraw et al. 1998. *Contemporary Economic Policy*.

The assessment in this manuscript relies on central estimates. In only one instance did I find a discussion of the importance of uncertainty or controversy. On page 11, in the context of the epidemiology, the paper states that other assumptions would not overturn "the conclusion that the benefits of Title IV exceed the costs." Such assessment would be helpful with respect to other ones of the estimates.

Especially prominent is the role of premature mortality, which constitutes 92% of the estimated benefits in Table 5. The VSL that is applied is $5.5 million. The manuscript might point out that the estimate of mortality related benefits moves linearly with this assumption. A plausible alternative of, say, one-third of this amount, reduces benefit estimates of premature mortality by $65 billion. The sensitivity of the 30-fold ratio of benefits to costs should be discussed in this context. Moreover, as stated already on page 11, it should be made clear that even at the lower bound of mortality benefits, the benefits are an order of magnitude greater than costs.

Another potentially major assumption is the emission reduction attributable to the program. Were the emissions forecast for the baseline reduced, the benefits of the program would be reduced directly. The detailed assessments of the cost side mentioned above conclude that an over-arching trend in the industry in the early 1990s was the introduction of low sulfur coal into Midwestern boilers, and that due to economics this would have occurred even in the absence of Title IV. Ellerman and Carlson calculate baselines and implied reductions that are about one-half of those calculated in this study, I believe. This would cut benefits in half. This point could be explored in the body text and perhaps again in the conclusion.

One study of ex ante estimates of cost is cited (Harrington et al.). However, I believe the major lesson of that study is not summarized correctly. On page 8 the paper states that these authors find technological change to be a key factor, and it is the only factor mentioned. This is only true indirectly. I believe they find that in general the emissions in the baseline are typically over-estimated, and consequently emission reductions are over-estimated, leading to an over estimate of costs. The same critique might apply to the present manuscript.

It is unclear from the discussion on page 5 and page 7 whether projections of emission changes (that is, the forecast of emissions in the baseline without Title IV) are new modeling results using IPM, or estimates based on some calculated factor. Can this be made more specific?

The second paragraph of the paper could be shortened perhaps, and combined with the full paragraph on page 4.

Key uncertainties in the prediction of emission changes mention facility life, and rates of adoption of new technologies (p. 6). However, looking back one finds major differences in the rate of expected demand growth also.

Page 8, top. "Current estimates …less than half the average cost predicted in 1990." Is this referring to the average of predictions, or prediction of average costs? Should this be marginal costs?

P. 10, middle. The paper should mention that since the particulate matter and ozone models are not integrated the results are not guaranteed to be consistent. For example, the forecast of ozone changes may not be consistent in a geographical area with the forecast of particulate changes. On average, presumably they are consistent, right? Why do we think so?

Page 15, top. The discussion reconciling the current estimates with previous ones is refreshing, but could be even more prominent in the paper. On page 15, top it is not clear what are "differences in the calculation methods". Also, the epidemiology is mentioned but there is no mention of uncertainty in valuing mortality, as noted above. McGuinness et al. 2003, *Land Economics* compare the major sources of uncertainty in the context of NOx benefits and costs.

Page 22, the rate of improvement in the ecology affected by acidification is described accurately, but I think there is some new evidence that is a bit more optimistic. Driscoll et al., 2003, *Environmental Science and Technology*.

Review MS #879-04

I have read MS #879-04 by Chestnet and Mills. It is a clear and well written description of an excellent policy assessment (benefit-cost analaysis) of Title 4 of the US Clean Air Act. I recommend that it be published. I do have some comments/suggestions for the authors to take into account in a revision.

1. How is new source review (NSR) modelled in the w/o Title 4 baseline scenario? Is it the Clinton Administration interpretation or the Bush revisions? This would make a difference in the estimated benefits. I do not have a strong preference for how they do it. But this ought to be clarified for the reader.

2. On the VSL, they should mention the review my Mrozeck (spelling? and Taylor, who suggest a lower figure than Viscusi and Aldy.

3. They shuld provide a citation to the literature on the possible oozone-mortality relationship for the reader who wants t learn more about this.

4. Do they want to at least mention the Hg and adult cardiovascular disease relationship? My only knowledge of this comes from a recent reading of the RFF Discussion Paper by Krupnick and others on the benefits and costs of Hg fish consumption advisories. In their analysis the adult cardiovascular disease relationship is the largest (as I recall) component of benefits.

5. I recommend reformatting Table 5 in a regular column as if it was going to be added up. This would make it visually clear that the recreation and ozone componennts are de minimus compared to the mortality benefits.

If these coments are returned to the authors, I prefer to be identified as the referee.

Sincerely yours,

Rick Freeman

Journal of Environmental Management

REVIEW

MS No. 879-04

Title: A Fresh Look a the Benefits and Costs of the U.S. Acid Rain Program

Comments:

This manuscript provides a thorough and competent compilation of available information on the costs and benefits of the implementation of Title IV of the Clean Air Act Amendments of 1990. The choices made among the options for the selection of data and models to be used in their analyses are clearly stated, and appear to be appropriate.

On the whole, the paper makes an excellent contribution to the literature at a time when the topics it addresses are increasingly important. It could have a significant influence on future clean air legislation and its implementation.

The following are some suggestions to the authors for modifications of the manuscript.:

Page   Paragraph   Line(s)   Comment

5   1   4   Change “new” to “1997”.

9   2   5   Update the reference to the 2004 draft of the PM CD.

10   2   4   The decision to confine the ozone analysis to the Eastern half of the US was not justified. This is especially important because of the large population exposed to ozone in Southern California.

10   3   3 & 4   The exclusion of ozone-related mortality needs to be justified here. It is true that ozone has not been implicated in excess annual mortality (Pope, et al, 2002), but peak levels do appear to have some effect on daily mortality that is independent of, and much smaller than the PM effects in some recent time-series studies.

Recommendation:

I recommend that this paper be published without change. _______.

I recommend that this paper be published with changes indicated. _______.

I recommend that this paper not be published. _______.

Changes to Manuscript 879-04 and Response to Comments

Lauraine Chestnut and David Mills, April 5, 2005

1. One of the reviewers asked for more recognition of uncertainty in the estimates. We have added a sentence acknowledging the overall uncertainty in the abstract and in the conclusions, along with the point made in the main text that even much lower estimates of benefits would still exceed the costs. This was a point that one reviewer asked that we give more emphasis.

2. We re-ordered the introduction as recommended by one reviewer.

3. As requested by two reviewers we added more information on EPA's baseline assumptions for estimating emissions without Title IV and other emissions/cost model inputs. We gave more information in notes and in text on the IPM modeling done for this analysis. We explained specifically the assumptions made regarding expected growth in electricity demand and in the amount of low-sulfur coal switching that would have occurred without Title IV. One reviewer emphasized the latter point and suggested that accounting for fuel switching that would have occurred without Title IV would cut the estimates in half. We have added a discussion of this issue and the citations suggested by the reviewer (Ellerman et al. 2000 and Carlson et al. 2000). We have added an explanation to the text that EPA's analysis has taken this into account, which is a change from the analysis EPA did 10 years ago. We added that this change reduced the emissions reduction attributed to Title IV by about a million tons. We don't agree with the reviewer's suggestion that this would cut the results in half.

4. We clarified the wording on page 8 in the comparisons of current cost estimates to previous cost estimates.

5. We revised the discussion of the Harrington et al. 2000 analysis to include their consideration of the effect of over-estimating the counterfactual baseline as a contributing factor to over-statement of costs. We don't quite agree with the reviewer's assertion that this is the main reason costs are overstated. As we read Harrington et al. we think they are saying this is one of several reasons, and they note that both unit costs and total costs were overstated for Title IV, which we have added to the text.

6. We added a paragraph on an earlier benefit-cost analysis of Title IV that one reviewer said should be acknowledged (Burtraw et al. 1998). We note that the approaches are conceptually similar, but there are several differences in the models used. A key difference in results is that Burtraw et al found a smaller reduction in emission attributable to Title IV. The reviewer suggests that this is because EPA did not account for low-sulfur coal fuel switching that would have occurred without Title IV. However, EPA did take this into account and still obtained a higher emissions reduction estimate.

7. Another difference with previous estimates of benefits of Title IV is the central concentration-response function used for PM mortality. We added more discussion and justification of exclusive use of long-term exposure studies for this, which is one of the reasons benefits results are higher than for previous analyses.

8. Two of the reviewers suggested more discussion of uncertainty specifically in the monetary valuation of mortality. We have added a paragraph on this including discussion of the citation mentioned by the second reviewer (Mrozek and Taylor, 2002).

9. We added discussion of cardiovascular health effects of mercury exposure and two citations for this, as one reviewer recommended.

10. We revised the ozone mortality estimates and added them to the central estimates. This is consistent with two recent publications, which we have added to the references, and is supported by one of the reviewers.

11. We added a paragraph on Driscoll et al. 2003 as suggested by one reviewer regarding reductions in acidification observed in the Adirondacks.

12. We revised Table 5 as suggested by one reviewer to have the numbers right justified. We also added ozone mortality into the list of primary estimates and revised the footnotes to explain the rounding and acknowledge uncertainty.

13. We updated the reference to EPA's PM Criteria Document, as one reviewer requested.