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Review of the Environmental Economics Research Strategy of the U.S. Environmental Protection Agency

A Report by the EPA Science Advisory Board Environmental Economics Advisory Committee

UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
WASHINGTON, D.C. 20460

OFFICE OF THE ADMINISTRATOR
SCIENCE ADVISORY BOARD

July 9, 2004

EPA-SAB-04-007
The Honorable Michael O. Leavitt
Administrator
U.S. Environmental Protection Agency
1200 Pennsylvania Avenue, NW
Washington, DC 20460

Subject: Review of the *Environmental Economics Research* Strategy of the U.S. Environmental Protection Agency; A Report by the EPA Science Advisory Board

Dear Administrator Leavitt:

This letter transmits the advice of the U.S. EPA Science Advisory Board (SAB) Environmental Economics Advisory Committee (Committee) on the U.S. EPA's *Environmental Economics Research Strategy*. The Agency asked the SAB to consider a number of issues, including whether the Research Strategy adequately characterized the major research gaps in key areas and addressed the issues of greatest scientific uncertainty; whether the research was feasible and likely to generate high quality results in a timely manner; what valuation methodology research should be investigated, whether the agency had missed any issues of overriding importance, and how best to communicate the research needs to the wider research community.

The Committee focused on five key areas of research; valuation of human health and ecological benefits of environmental improvement, environmental behavior/decision-making, market methods/incentives, and benefits of environmental information disclosure.

In general, the Committee concluded that the Research Strategy adequately characterizes the major research gaps in the literature for the benefits of human health and ecological risk reduction and that the research could generate high quality, useful information in a reasonable time frame. The Committee noted that valuation of human health risk reduction benefits should consider the reliability of existing estimates of the value of

statistical life and that research should also be conducted on the issue of marginal versus non-marginal risk valuation. In addition, the Committee emphasized that to be most valuable, there must be a high level of interaction between economists and ecologists in the area of ecological benefits research.

The Committee noted that in spite of the focus of the academic literature on nonmarket valuation methodologies, some areas of valuation still would not receive appropriate funding without EPA attention and interest. The Committee listed a number of examples of methods research that would be of importance for the agency.

The Committee provided information on several environmental compliance and decision making issues and noted that this is one of the most important research topics for the agency. They strongly urged that research should focus on overall environmental behavior, not just compliance. The Committee stated that the ultimate goal in this area is to understand why firms differ in the amount of pollution they create (or "in pollution intensity.") A number of additional research topics were identified by the Committee.

In the area of market mechanisms and incentives, the Committee believed that the Agency has identified some extremely important areas for future research and that the research could lead to high quality results. However, the Committee judged the proposed research to be too limited in its focus and they identified some additional areas for Agency consideration.

The Committee agreed with the Agency on the importance of environmental information disclosure and agreed that there is no generally accepted method for estimating the benefits of these programs. A first step in estimating the benefits of such programs is to accurately estimate their impacts on emissions and ambient pollution. Additional research in this area is certainly required. The Committee also suggested that the agency contrast the cost-effectiveness of environmental information disclosure with the cost-effectiveness of traditional approaches to pollution regulation. The Committee noted that studies of the effectiveness of information disclosure under the Emergency Preparedness and Community Right-to-Know Act of 1986 could provide valuable information regarding the viability of such policies to reduce the environmental component of terrorism risk.

Finally, the Committee recommended that EPA could achieve wider distribution of the results of the research strategy and receive useful feedback from members of the research community by holding workshops in conjunction with the annual meetings of the American Economics Association and the American Agricultural Economics Association.

We appreciate the opportunity to review, and to provide you with advice on, the EPA's draft *Environmental Economics Research Strategy*. We look forward to your response to this report.

Sincerely,

/Signed/

Dr. William H. Glaze, Chair
EPA Science Advisory Board

/Signed/

Dr. Maureen Cropper, Chair
Environmental Economics Advisory Committee
EPA Science Advisory Board

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Review of the *Environmental Economics Research* Strategy of the U.S. Environmental Protection Agency: A Report by the EPA Science Advisory Board

1. INTRODUCTION

1.1 Background

This report transmits the advice of the U.S. EPA Science Advisory Board (SAB) on the EPA's *Environmental Economics Research Strategy*. This report was prepared by the Environmental Economics Advisory Committee subsequent to its review which began during November, 2003. The review was announced in the *Federal Register* (see 68FR61206).

1.2 Charge to the Science Advisory Board

The Environmental Economics Advisory Committee (EEAC) was requested to review EPA's Environmental Economics Research Strategy. Specifically, the EEAC was asked to address the following charge questions:

1. For each of the major subject areas described in the EERS, EPA has attempted to articulate the research questions most relevant to EPA that can be effectively addressed given the available tools and resources. In this context, please address the following for the key research questions identified in the EERS in each of the subject areas.

    a) Is the characterization of each of the major research gaps in the literature for the key subject areas of relevance to EPA's economic sciences, as identified in the EERS, adequate? Will these priorities and implementation approaches effectively address the areas of greatest scientific uncertainty?

    b) Given the implementation strategy laid out in the EERS;

    --To what extent is this research scientifically feasible at a high level of quality?

    --How successful is this research likely to be in answering policy-relevant questions for EPA within the next 8-10 years?

    c) What improvements in the design and implementation of the EERS would make each research project more useful to EPA and other environmental management agencies?

2. What methodological research needs in valuation should EPA investigate as a complement to the needs derived from the strategy interviews? In the valuation areas, EPA's expressed needs are primarily practical: better values for ecological and human health impacts of environmental policies. However, most grant proposals (and most journal articles) investigate practical questions as well as methodological or other questions (e.g. incentive

compatibility or elicitation methods in stated preference or more refined models of behavior in revealed preference). EPA does not expect that researchers will propose to estimate only the practical values that EPA needs, but will also propose to investigate methodological issues. Since the research strategy interviews did not elicit methodological needs, and EPA believes that improving methodology while generating practical values provides useful synergy, further input on prioritizing methodological issues from the EEAC would be useful.

3. Can the SAB identify by consensus any environmental economics issues of overriding importance to EPA that the EERS has missed, and that EPA should address provided that more resources would be available for Environmental Economics Research? Could the SAB explain why this (these) issue(s) should be of high concern to EPA's research programs.

4. What is the best way for EPA to communicate the results of the research strategy and plans for achieving its long-term research goals to the wider research community, and other potential users?

In addressing these questions the committee focused on the five key areas in which EPA identified additional research needs:

a) Valuation of the human health benefits of environmental improvements
b) Valuation of the ecological benefits of environmental improvements
c) Environmental behavior and decisionmaking
d) Market methods and incentives
e) Benefits of environmental information disclosure

1.3 Format of this Report

The Science Advisory Board's answers to Charge Questions 1 and 2 are presented by research area. The discussion begins by summarizing EPA's research priorities in each area, and then answers Charge Questions 1 and 2. Charge questions 3 and 4 are answered at the end of document.

2. ANSWERS TO CHARGE QUESTIONS 1 AND 2

2.1 Valuation of Human Health Benefits of Environmental Improvements

2.1.1 Characterization of Research Gaps and Priorities

Is the characterization of each of the major research gaps in the literature adequate? Will these priorities and implementation approaches effectively address the areas of greatest scientific uncertainty?

Most of the literature on human health valuation has focused on mortality valuation—measuring what individuals will pay for small reductions in their risk of dying in a specified time period. As the EERS notes, much less effort has been invested in measuring what people will pay to avoid episodes of acute illness or to reduce their risk of contracting a chronic illness (morbidity valuation).[1] As a result, many health endpoints are not monetized in Regulatory Impact Analyses; or their value is measured by productivity gains and avoided medical costs, which are a lower bound to the appropriate measure of value. In the area of morbidity valuation, the EERS (p. 10) calls for valuation of non-cancer endpoints; earaches, headaches, colds, gastrointestinal upsets, reproductive and developmental effects (e.g., Attention-Deficit Disorder, autism), and cancer-related morbidity effects. On p. 15 of the research strategy, more emphasis is put on chronic health effects. The EERS notes that it is important to allow valuation to depend on severity, duration and frequency of symptoms. Emphasis is also put on valuing children's health.

According to the EERS, the main needs in mortality valuation (p. 16) focus on the impact of age on the Value of a Statistical Life (VSL) and on the impact of the health status of the individual and co-morbidity (illness preceding death) on the VSL.

The committee agrees with the Agency's assessment on the lack of estimates of the value of morbidity risk reduction benefits. Willingness to pay (WTP) estimates are indeed unavailable for many cancers, as well as for risks of heart attacks and strokes. WTP estimates for the other health endpoints noted in the first paragraph are also lacking. In many cases, the productivity losses and medical costs associated with illness are not born by the individual. If this is the case, estimates of productivity losses (the so-called indirect costs of illness) and medical costs (the direct costs of illness) must be added to WTP estimates. Estimating these illness costs is itself a research task whose importance should not be underestimated.

Regarding the Agency's priorities in the area of mortality valuation, the impact of age on the VSL is an important, policy-relevant topic. Any environmental regulation that saves lives in proportion to the age distribution of deaths in the U.S. will primarily extend the lives of older people. (Fifty percent of the deaths in the U.S. occur after age 75.) Regarding the impact of health status and co-morbidity on the VSL, the important issue here is whether environmental pollution causes chronic illness or simply increases mortality risk for people

[1] WTP estimates exist for reduced risk of chronic bronchitis and for some cancers, as well as for asthma attacks, restricted activity days and symptom days.

who have pre-existing chronic conditions. When environmental contaminants cause a disease, it is theoretically desirable to value the risk of contracting the illness (such as cancer) which will entail both morbidity and a reduction in life expectancy. If pollutants differentially affect mortality risk for those with a pre-existing condition (e.g., cardiovascular disease), then efforts should be put on measuring the impacts of the health state on the value of increasing life expectancy.

There are three topics in the area of mortality risk valuation that the committee believes deserve attention even though they are not mentioned in the EERS. The first concerns the reliability of existing estimates of the VSL, which rely on labor market and on stated preference studies. The Agency has recently commissioned re-analyses of data from compensating wage studies (Black, Galdo and Liu 2003) and of data from contingent valuation studies of mortality risks. Examination of these results may suggest that emphasis should be placed on developing newer, more reliable estimates of the VSL.

The second research topic concerns the issue of marginal versus non-marginal risk valuation. Emphasis in the literature is on valuing small changes in risk of death, on the order of 1 in 10,000 per year or smaller. The agency, however, uses these estimates to value regulations that, together, account for much larger risk reductions. To illustrate, *The Benefits and Costs of the Clean Air Act 1970-1990* (USEPA 1997) predicted that air quality regulations issued between 1970 and 1990 reduced mortality by 200,000 lives in 1990. When a VSL of $4.8 million (1990 USD) was applied to these statistical lives, the WTP in 1990 for mortality risk reductions occurring in that year was estimated to be approximately $16,000 (1990 USD) for a family of four.[2] This was because the VSL was applied to a non-marginal reduction in risk of death (on average, a 1/1,000 reduction). Similarly large benefit estimates may arise when one adds together WTP for the risk reductions associated with the 1990 Clean Air Act Amendments, the Tier II Emissions standards, the Heavy Duty Engine Diesel Rule, the Off-Road Diesel Rule and Clear Skies. In short, while a single regulation may confer marginal risk reduction benefits, the set of environmental regulations evaluated over a 10-year period may confer non-marginal benefits, and should be evaluated accordingly.

The third research topic concerns the impact of income on the VSL. Historically EPA has adjusted future values of the VSL to allow for income growth. Such adjustments require estimates of the income elasticity of WTP for mortality risk reductions. We believe that this is a topic that requires additional research.

2.1.2 Research Feasibility

Given the implementation strategy laid out in the EERS; --To what extent is this research scientifically feasible at a high level of quality? --How successful is this research likely to be in answering policy-relevant questions for EPA within the next 8-10 years?

[2] The total value of the statistical lives saved, $1 trillion, when divided by the population of the U.S. in 1990, implies a WTP of approximately $4,000 per person.

There are several criteria to consider in formulating a research strategy for morbidity valuation:

a) The approach taken should be able to evaluate a large number of health endpoints.
b) It should, ideally, value the *risk* of avoiding chronic illness rather than avoiding the illness with certainty.
c) The definition of health endpoints should take into account the severity, duration and frequency of symptoms.
d) The health endpoints valued should correspond to those linked to environmental pollutants in the epidemiological literature.

An important question, related to mortality valuation, is whether chronic morbidity and mortality should be valued as part of the same package. For example, should people value the risk of developing congestive heart failure as the risk of experiencing a series of hospital episodes, recurring discomfort and shortened life span? Or, should the impact of air pollution on the morbidity associated with congestive heart failure be valued separately from the impact of air pollution on the mortality associated with congestive heart failure? The answer to this question depends crucially on the point articulated in item 'd' above. If the epidemiological literature suggests that air pollution increases risk of death due to congestive heart failure (and other forms of heart disease), but does not link air pollution to increased incidence of the disease, then valuation should mirror this approach. For diseases where environmental contaminants may actually increase the risk of contracting the disease (e.g., for some cancers) then, assuming that people can comprehend both the risk of the disease and its sequelae, the goal should be to value the morbidity and mortality risks as a single package.

Regarding the number of health diseases to be valued, a possible approach to dealing with the point articulated in item 'a' above is to have people value functional limitations (characterized by severity, duration and frequency) which, in turn, are related to illnesses. This is likely to work best for chronic illnesses and less well for acute illnesses. Obtaining reliable values for the *risk* of an illness will remain a challenge. One potential area of research is to examine whether it is possible to use information contained in measures of health preference, such as quality adjusted life years (QALYs), that are used in other public-health domains to provide a method for transferring benefit estimates from one health endpoint to another.

Regarding estimates of the value of mortality risk reductions, it would seem that stated preference methods are the main candidates for studying the impact of age and health status on WTP, although it may be possible to use revealed preference methods for the impacts of age in the context of auto safety. (Labor market data probably cannot be used to estimate WTP for ages greater than about 65 because of the relatively small fraction of the elderly who are employed (Viscusi and Aldy 2003)). The use of both stated and revealed preference methods to value reductions in risk of death is advisable. Regarding the issue of valuing non-marginal risk changes, this is certainly feasible using stated preference approaches. Recent advances in dealing with the identification problem in hedonic markets suggests that this should also be feasible in a revealed preference context (Heckman, Matzkin and Nesheim 2003).

The committee believes that progress in the area of health valuation can be made, with appropriate funding, within the next 8-10 years. We believe that for these efforts to be successful, and to yield high-quality research, it is essential that the economists conducting the research work together with epidemiologists to ensure the that health effects valued match up with those examined in the epidemiological literature, and with physicians to ensure that any health status indices that are used to facilitate evaluating a large range of health endpoints are appropriate to the task

2.1.3 Usefulness of the Research

What improvements in the design and implementation of the EERS would make each research project more useful to EPA and other environmental management agencies?

The committee believes that it is important to measure the impact of household characteristics on WTP for both morbidity and mortality benefits. This is important if the Agency is to examine the distributional impacts of environmental policies. It is also the case that certain air pollution epidemiology studies distinguish health effects by socioeconomic status (e.g., Pope et al. 2002). Making these distinctions in terms of valuation would make it possible to further refine the distributional impacts of air pollution control strategies.

2.2 Valuation of Ecological Benefits of Environmental Improvements

2.2.1 Characterization of Research Gaps and Priorities

Is the characterization of each of the major research gaps in the literature adequate? Will these priorities and implementation approaches effectively address the areas of greatest scientific uncertainty?

The EERS identifies a number of endpoints for which valuation of ecological services will be useful. These endpoints include water quality changes, ecological impacts of air pollution, introduced versus native species, avian species, etc. While this way of thinking about the benefits of ecosystem improvement is consistent with how economists view the valuation of many goods, this single endpoint focus is not generally consistent with how ecologists view ecosystem functioning. For research in ecological benefits to be most valuable, there must be a high level of interaction between economists and ecologists. To begin, there must be a broad understanding of the way in which ecologists view ecosystem functioning. For example:

a) The research plan refers to "outcomes" as if ecosystems are mechanical," i.e., when the level of an insult increases the ecosystem has predictable new equilibrium outcomes. This characterization of ecosystem dynamics may be appropriate in some cases, but this is not the way ecologists think about ecosystem responses generally. Rather, they tend to think of ecosystems as constantly changing, with different levels of insult affecting the course of change. Even if there is an equilibrium, the equilibrium may not be reached for

decades or a century, so outcomes have to be thought of in a more dynamic way. On the other hand, ecologists do think of the services provided by well functioning ecosystems as something that can be characterized and assessed. This may be the point at which ecologists and economists can best find common ground.

b) Ecologists are more concerned with thresholds and irreversibilities in ecosystems than in the pros and cons of changing from one equilibrium to another. One of the effects of a change may be that the ecosystem is less resilient to the next insult, so alternative scenarios of future insults need to be explored, not one-time changes.

c) This brings us back to earlier efforts to tackle cumulative effects. Ecologists do not think of stresses as being separable. Hence they are more "conservative" than economists who are comfortable with the assumption that stresses are separable and hence can be considered "on the margin." For ecologists, the order of stresses also can be important. The earlier efforts of economists to understand cumulative impacts should probably be reviewed as a starting point for moving forward in this respect.

While it would be nice if ecologists could simply provide predictions of ecosystem behavior to meet economists' specifications of what they need, the fact of the matter is that ecologists are the experts on ecosystems. Implying that the economists need something that is contrary to the expertise of ecologists may not be a good starting point for what has to be a joint project. The research strategy should address how economists and ecologists in the Agency can learn more about what each other knows and how they can develop shared understandings, language, and models. Rather than valuing changes in a single service from an ecosystem, e.g., increased avian species or increased fish catch, it may be appropriate to work with ecologists to value the entire bundle of service changes associated with a change in an ecosystem from one state (perhaps one with a low level of function generally) to another (one that supports on average more species diversity, more resilience to external shocks, etc.).

Research on the benefits of ecological systems and services will necessarily need to be ecosystem specific. While "outcomes" for human health effects are many and are probably sensitive to the age at which the problem occurs, they are going to be pretty much true whether a person lives in Phoenix or Philadelphia. For ecosystems, while there are classifications of ecosystem types that will prove helpful, there are going to be a considerable number of types. The condition of the ecosystem for which an additional stress is being considered also will be important. Of course, the conditions of people experiencing an environmental stress are also important, but the stress is generally experienced by a large population of people whose conditions can be understood in statistical terms. Decisions on ecosystems will much more likely be taken one system at a time.

The EERS recognizes the large reliance the agency places on benefits transfer and the likely need to continue this reliance. They suggest that a "cost-effective strategy may be to investigate methods that generate theoretically sound values for multiple endpoints." The

committee generally agrees with the logic of this idea, but the Agency should consider that the highly contextual nature of ecosystem services may make transfers even more challenging than when single endpoints can be focused upon. Benefits transfer in the case of ecosystems is exceptionally challenging.

One important issue noted in the document is the fact that ecosystem services and functioning are not well understood by the general public, thus complicating the process of valuation. In addition to eliciting the value of ecosystem changes, the valuation process usually requires some education of the public. This has important implications for the resulting values and for the resulting use of benefits transfer.

The relationship between physical measures of ecosystem functioning (pollution concentrations in water or air, number of species present in a land area, etc), features of an ecosystem that people perceive and the perception of the ecosystem by people is poorly understood. Since this linkage is critical for valuation, this is an area of research that might yield high returns to EPA and other agencies.

The inherent uncertainties in ecosystem functioning make the need to characterize uncertainty in the valuation process particularly important when ecosystem values are sought. This is an area where additional work could be focused. Likewise, additional work on the implications of the "precautionary principle" and cost-benefit analysis when irreversibility and uncertainty is present may prove beneficial. Another feature that merits focus is the implication of threshold effects and valuation of large rather than marginal changes. Valuation for dynamic systems and the consequences of valuing changes in ecosystem services under very long time scales are also issues that EPA may wish to consider. Again, these features may be unique to ecosystem valuation as distinct from health endpoints or other nonmarket goods.

Among nonmarket values for ecosystem services, nonuse values are the poorest understood, yet they have the potential to be very large in magnitude. This implies that research that focuses on nonuse values may have the highest returns.

Some committee members felt that materials damage and losses of visibility from pollution should also be included among environmental benefits to be studied.

2.2.2 Research Feasibility

Given the implementation strategy laid out in the EERS; --To what extent is this research scientifically feasible at a high level of quality? --How successful is this research likely to be in answering policy-relevant questions for EPA within the next 8-10 years?

The committee sees considerable return on research in the area of ecosystem valuation. With adequate funding, the Agency is likely to see information that is very relevant for policy questions being produced.

2.2.3 Usefulness of the Research

What improvements in the design and implementation of the EERS would make each research project more useful to EPA and other environmental management agencies?

Many of the ecological benefits measures needed by EPA would also be of value to other government and public agencies. Specifically, the U.S. Army Corps of Engineers, USDA, Forest Service, and many Nongovernmental Organizations and state agencies would find ecological benefits work to be highly valuable. EPA should find ways to share research results as well as consider jointly funding and identifying needed research on ecological benefits.

One of the needs identified in the report is for valuation of reductions in pesticides in water, but in many parts of the country, nutrients are as much or significantly more of an issue. Nitrogen is a clear problem for hypoxia, while phosphorous and other nutrients are important for local water quality problems in much of the Midwest.

A centralized team of economists in Washington D.C. may not be the ideal configuration of economists for undertaking research that is going to be inherently more contextual than other types of environmental economic research. More economists are probably going to be needed in the regional offices to carry out ecosystem valuation effectively. This activity could also be enhanced by the use of non-EPA economists (outside of Washington) with expertise in ecosystems analysis.

2.3 Valuation Methodologies

What methodological research needs in valuation should EPA investigate as a complement to the needs derived from the strategy interviews?

As noted in the Research Strategy much of the academic literature in nonmarket valuation focuses on methodological development. While this implies that a substantial amount of methodological work may be produced without impetus from EPA or other agencies, there are some areas of valuation where the needed methodological refinements may not be as forthcoming as others. In particular, some areas may not be viewed to be as methodologically interesting as others, but may still be critical to improving the accuracy of nonmarket valuation in the context of ecosystem services. The committee identified the following topics as being of particular value to EPA programs:

a) Improved methodologies and tools for benefits transfer as well as increased understanding of the range of uncertainty associated with the transfer of values from one study site to another. Given the enormous reliance that EPA and other agencies make on the transfer of benefits to perform benefit-cost analysis, considerable gains from additional understanding of this process may be possible.

b) Increased understanding of the appropriate extent of the market when taking welfare estimates from individual values and aggregating them to the full population (this is

particularly important for nonuse values). There is both the issue of having the appropriate sampling frame from which to compute per household benefits (e.g., a random population survey versus a sample of people visiting a shopping mall) and the issue of how much of the general population to apply these values to (e.g., households in close proximity to the impacted resource, all households in a certain political jurisdiction, all households in the world?).

c) Further study of the WTP-WTA divergence literature. In many cases a strong argument may exist for the use of a WTA valuation rather than WTP based on explicit or implicit property rights.

d) Increased understanding of the appropriate valuation of the opportunity cost of time in revealed preference studies. This concern relates primarily to recreation demand studies, where different assumptions about the value of time can yield differences in the estimates of welfare changes by a factor of two or more.

e) Improved bid design methods for stated preference surveys. Discrete choice stated preference surveys require the researcher to define the bid distribution for respondents. Potential bias and inefficiency from suboptimal bid design are not well understood.

f) While a growing literature in the valuation field combines stated and revealed preference information to jointly estimate the parameters of welfare measures, there may be substantial gains to EPA and other agencies from furthering work in this area. The combination of stated and revealed preference data enjoys the prospect of grounding welfare estimates in observable behavior while allowing for a much broader category of benefits to be studied with the inclusion of stated preference information.

2.4 Environmental Compliance Behavior and Decision-Making

2.4.1 Characterization of Research Gaps and Priorities and Usefulness of Research

Is the characterization of each of the major research gaps in the literature adequate? Will these priorities and implementation approaches effectively address the areas of greatest scientific uncertainty?

What improvements in the design and implementation of the EERS would make each research project more useful to EPA and other environmental management agencies?

This topic is one of the most important research priorities faced by the EPA. Judging from the survey of EPA program offices, this topic is tied with "Valuation of Reduced Morbidity Benefits" for the highest priority. Several research questions are highlighted, including: a) Why and how do facilities comply; b) What policies or approaches increase compliance; and c) How successful have voluntary programs been? The Committee offers the following comments on the research goals:

a) Previous EPA funded results. It would be helpful to assess how successful the previous Science To Achieve Results (STAR) competitions on Corporate Environmentalism have been.

b) Breadth of research area. This research priority area is termed *Environmental (Compliance) Behavior and Decision-making.* It is unclear whether the focus is limited to compliance-related behavior or whether this priority area is intended to include broader issues relating to environmental behavior. Some of the specific questions raised in this context (e.g., location, technology adoption, and pesticide use) do not involve compliance directly. They are questions about how firms make decisions that have environmental implications. The Committee believes that this is the correct perspective on this issue. Ultimately, EPA's goal should be to understand why some firms pollute more (per unit of input or output) than others. This may depend on the nature or environmental regulations and their enforcement, but will also depend on the costs of reducing emissions.

If the focus is on compliance alone, then it is not clear why the questions relating to voluntary programs are included, since by definition voluntary programs do not entail "compliance" with existing rules or regulations. The study of voluntary programs would fit more closely with the research about information disclosure. Alternatively, if this priority area is intended to be broader, then the research on information disclosure could be folded into this priority area.

c) Feedback from regulated industries. Traditionally, research on compliance and enforcement has treated the policy process as linear. The regulator first designs and imposes a policy, then an enforcement strategy (e.g., an audit frequency and penalties for non-compliance), and then the polluting firm decides whether or not to comply. Some research on enforcement and compliance might benefit from considering a more interactive model, under which regulators and regulated parties work together to identify pollution sources and means to address them. Monitoring and information generation are key parts of such a strategy. One example is EPA's Clean Charles 2005 Initiative. (Again, this suggests that the research priority area on information disclosure is closely linked to this priority area.)

d) Public sector compliance. Most of the compliance and enforcement literature studies private-sector polluters. Other sources, such as municipalities, have received relatively little attention. Yet in some contexts these constitute the primary pollution sources (e.g., water pollution). Thus, the scope of the research priority should be sufficiently broad to include not only decisions by private polluters but also by public sources.

e) Unobserved determinants of behavior. The existing literature has understandably focused on easily observable determinants of environmental decisions: firm size, industry group, etc. However, less easily observable motivational factors may be more important in explaining the environmental behavior of a particular firm. Information about these factors comes primarily from case studies, which are often

viewed as anecdotal. Therefore, it seems appropriate for EPA to fund research that seeks to provide statistical evidence regarding firm decision-making, as well as more in-depth case studies of the behavior of individual firms.

f) Market incentives. A key question that needs to be addressed (statistically and through case studies) is whether market incentives for environmental protection (e.g., from consumers, communities, suppliers, or investors) are sufficiently strong. Again this relates closely to the effect of information disclosure, since information disclosure is likely to increase these pressures. Instead of thinking about voluntary approaches and information provision as substitutes for more traditional regulations, they may be thought of as complements.

g) Cost-effectiveness. Most studies focus on accounting costs of compliance, and ignore things like the transactions costs of dealing with regulators, liability costs, and adverse publicity. It would be worth considering these broader definitions of costs.

h) Market structure. Regulations may have different effects on industries with different market structures.

i) Appropriate fines. The EPA's current practice requires firms caught violating environmental regulations to pay fines equal to the profits they earned as a consequence of their violations. It is an open research question whether this is in any way optimal. Research could also be conducted on the use of resource-based compensation in lieu of fines. Sometimes violators can avoid fines by undertaking restoration or enhancement activities of great value than the fine. The desirability and effectiveness of non-monetary penalties need to be better understood.

j) *Ex-ante* versus *ex-post* estimates of compliance costs. *Ex-ante* estimates are typically higher. This may be due to strategic industry overstatement of costs, or because technological improvements are not foreseen. Research on the difference between *ex-post* and *ex-ante* estimates would be useful to researchers assessing future proposed regulations.

k) Data. Several researchers noted that state variation in approaches, fines, monitoring, etc., can be used to study these issues, but that state data are difficult for individual researchers to collect. A useful role for EPA may be to encourage states to collect data in a standardized way and to assist in compiling the data.

2.4.2 Research Feasibility

Given the implementation strategy laid out in the EERS; --To what extent is this research scientifically feasible at a high level of quality? --How successful is this research likely to be in answering policy-relevant questions for EPA within the next 8-10 years?

In general, Committee members felt that EPA researchers have identified an important area for research, and one that could lead directly to improvements in EPA efficacy, reductions in regulatory costs, and improvements in environmental quality. Key tasks for the EPA include: defining the scope of the research objective, assessing prior EPA funded research, and assisting with the unification and publication of monitoring and enforcement data.

In addition, the Committee noted that any research in this area will have to deal with the following problems:

a) Enforcement actions are widely understood to be targeted at likely violators, and hence endogenous. Researchers cannot estimate the effect of inspections and enforcement on the probability of violating environmental laws without simultaneously estimating the effect of violations on enforcement. This task is difficult without knowing the procedures EPA or the states use to decide which facilities to inspect. For a seminal paper on this topic, see Magat and Viscusi (1990).

b) A second empirical problem, noted by Harrington (1988), is that given the low probability of any particular firm being inspected, or punished given an observed violation, the overall level of compliance is surprisingly high. Some other phenomenon aside from regulatory enforcement must explain compliance: public relations, citizen suits, NGO actions, etc.

c) A third empirical problem involves the availability of data. Much of the empirical literature focuses on the pulp and paper industries, due to the availability of water pollution enforcement data via the Permit Compliance System (PCS).

d) Finally, this section of the research strategy asks to what extent voluntary pollution reduction programs such as the "33-50" program have succeeded. Answering this question requires facing another simultaneity problem: firms that have unobserved tendencies to reduce pollution are more likely to volunteer to do so. To accurately assess the efficacy of these voluntary programs, a researcher will need some exogenous variation in the programs, or some instrument for program participation. The Agency needs to be particularly attentive to opportunities to exploit exogenous variation in eligibility for particular programs so that valuable chances to assess the consequences of “natural experiments” are not missed. Labor economists have sensitized the discipline to the desirability of natural experiments for program evaluation. In particular, the Agency needs to be watchful whenever there are boundaries in some dimension (time, firm size, space) across which the assignment of firms to regulatory regimes is randomized by arguably external factors.

2.5 Market Methods and Incentives

2.5.1 Characterization of Research Gaps and Priorities and Usefulness of the Research

Is the characterization of each of the major research gaps in the literature adequate? Will these priorities and implementation approaches effectively address the areas of greatest scientific uncertainty?

What improvements in the design and implementation of the EERS would make each research project more useful to EPA and other environmental management agencies?

The Research Strategy combines the research priority "Market Mechanisms and Incentives [MMI], Trading" with the priority "Market Mechanisms and Incentives, Other than Trading" in its final list of research gaps (p. 19). The proposed research emphasis from the Office of Research and Development will be on "trading in practice and trading in new markets" (p. 29). These specific areas are mirrored in the identified research gaps on p. 19. Especially regarding "trading in practice," the key questions that are identified are the environmental effects of trading and estimating the resulting cost savings. For new applications, the key questions are predicting the success of new markets and designing the markets to achieve both environmental and cost-reduction goals.

Some specific possible research areas that are mentioned include using market approaches for urban storm water management; programs for new pollutants and media; the complexities of tradable water quality permits in a world of multiple market distortions, lack of monitoring, and "cultural resistance to enforcement" (p. 20); the interactions of marketable permits with existing taxes; and market design questions.

The Committee believes the Research Strategy identifies some extremely important areas for future research but is too limited in its focus. The following are also areas worthy of inclusion in the MMI research program.

a) Market mechanisms other than trading are notably absent from the discussion of research gaps. Exceptions include the brief mention of "environmental information programs" (p. 12) and "Methodology for evaluation of effectiveness of voluntary programs" (p. 26), which are already the subject of research priorities identified elsewhere in the EERS. The exclusion of other MMI instruments, or more generally incentive-based instruments, such as pollution taxes, abatement subsidies, scrappage schemes for old cars, and deposit-refund systems, unduly limits the policy approaches that merit study. For example, current environmental policy relies heavily on legal liability for environmental contamination, which is an incentive-based policy. The empirical effects of these policies are not well understood but are a promising area for future research because data exist from recent experience.

Although the language in the Strategy does not rule out study of mechanisms other than permits, the absence of their mention does not encourage it either. Research has shown that different market-based instruments vary in, for instance, their effects on technological change, their total costs to pollution sources, their effects on entry and exit in an industry, the potential for "double dividend" effects, and the political or social acceptability of the instruments. Exploration of the relative merits of other instruments would be a valuable area of study.

b) The spatial and temporal effects of MMI also need further attention. For many, if not most, pollutants, the kind of cap-and-trade program exemplified by the acid rain program differs from the design of a trading program identified in economic theory because the spatial and temporal distribution of pollution determines pollution damages. For efficiency, the marginal damages at any given locale or time need to be considered. Even in the absence of information on marginal damages, a cost-effectiveness measure should take spatial and temporal effects into account, due to requirements in environmental laws to achieve site-based environmental targets (such as the National Ambient Air Quality Standards under the Clean Air Act). There has been little ex post assessment of the spatial effects of uniform trading ratios in cap and trade systems such as the 1990 Clean Air Act Amendments, the Ozone Transport Region's NO^x budget program, and the NO^x SIP call trading program.

The spatial and temporal effects of MMI are important components for design of future programs as the nation addresses mercury and ozone as atmospheric problems and the use of TMDLs in water quality. Simple trading designs, such as a cap-and-trade system for marketable effluent permits, may not achieve environmental targets in all places. On the other hand, incorporating transfer coefficients, limiting trading regions, or otherwise accounting for spatial and temporal effects limits trading opportunities and thus reduces potential cost savings. Research should evaluate the tradeoff between achieving environmental goals and achieving cost reductions, and the administrative and scientific difficulties involved in more theoretically correct trading systems.

c) The implications of monitoring and enforcement for the design of MMI policies should be an important part of the MMI priority. Monitoring can take at least two forms: one form measures actual environmental outcomes (e.g., ambient air or water quality), and another form ensures that the emissions or other factors for which a market is developed are measured at the source. Adequate monitoring of ambient measures can help tremendously in ensuring the success of MMI programs in achieving environmental goals in all locales. Research into ambient monitoring and the interaction with MMI requires involvement of environmental scientists, for example, to help identify the efficient placement and number of monitoring sites for achieving ambient goals. Monitoring of source behavior is necessary for the success of the markets: for example, if it is easy for sources to emit more than the number of permits they own, then the permit market as well as environmental quality will suffer.

d) An important and controversial aspect of MMI programs has been the emergence of trading without a cap on aggregate emissions. Such programs have been labeled "open market trading programs" because they allow for new participants who may identify low-cost opportunities for emission reductions to enter the program. These programs have been widely criticized because of the absence of a strict emission cap, and the difficulty in observing and monitoring emission reductions. However, this generic approach has a broad application in the Clean Development Mechanism and Joint Implementation aspects of international efforts to reduce greenhouse gases, and it continues to be suggested at the state level in the US for control of conventional pollutants. Open market trading should be studied to identify its weaknesses (so that programs can be designed to avoid them), its track record, and its role as a transition to cap-and-trade programs.

e) Another important area of study is the use of market approaches on indirect measures of the environment. For instance, it is typically almost impossible to measure nonpoint source pollution from a source, because the runoff cannot be observed. Instead, pollution policies are sometimes proposed for related goods, such as fertilizer use or Best Management Practices. Other examples include taxing gasoline instead of auto emissions and pay-at-the-pump auto insurance. When market instruments are applied to indirect measures, the environmental effects are much less understood; indeed, it is possible that adverse environmental outcomes might arise from unexpected substitutions or other unexpected effects.

f) The interaction of MMI with existing taxes and other policies (such as agricultural programs) is an important area that deserves study. The General Theory of the Second Best suggests that improving one market in a world of multiple distortions may not improve welfare; hence, it is worth understanding whether the use of MMI might lead to adverse effects in unexpected ways and how the design of policies can be improved in this light. We would accord priority to three particular manifestations of the second best and the importance of pre-existing policies that have been found to be very significant in previous research.

One has to do with policies or subsidies for such areas as agriculture or energy, outside EPA's jurisdiction, whose potentially significant environmental effects sometimes conflict with the goals of environmental policy. Other programs may have beneficial effects, and EPA may be able to learn from experience of programs in these other areas. For instance, the use of environmental targets in the USDA's Conservation Reserve Program deserves study as an important application of subsidies promoting, or consistent with, environmental objectives of the EPA. This program and its Environmental Benefits Index might be modified to target water quality and achievement of TMDLs. Generally, there could be significant environmental and financial gains from greater cooperation and coordination of research and policy across agencies.

A second area is pre-existing tax or regulatory policy. EPA has previously funded work on the "tax interaction effect" and found this to be significant. Significant opportunity exists to improve this research by making it more accessible for policy makers and by introducing greater specificity and heterogeneity in the analytical and simulation models that have been employed to date. In general, further research that addresses the value (in a public finance context) and potential uses of revenues from environmental policies would be a very important contribution to policy design in general. For example, mandatory emission fees in severe nonattainment areas can be used to generate revenues to subsidize investment in emission control by firms or in infrastructure improvement that will reduce emissions.

Finally, state-level policies may interact in unforeseen ways with federal programs. For instance, states have adopted renewable portfolio standards (RPS) as a way to influence technology choice and environmental performance in electricity generation. State-level policies, such as RPS or state emission restrictions, when implemented under the umbrella of an aggregate emissions cap at the federal level, could be ineffective for achieving national emission reductions, as state-mandated gains are traded away.

g) Another important area for future study that is excluded from the EERS is the implication of MMI designs for the perception of equity or fairness, and ultimately political feasibility of environmental policies. Economic research is often oriented toward measures of efficiency, but it also offers the tools to identify the distributional effects of policy. Of special interest and potential contribution is the identification of the distributional effects associated with different types of policy design for a given environmental goal. This research could measure the effects of policies on market value of firms and the distribution of damage from existing environmental burdens (and, implicitly, the distribution of benefits from improvements). Furthermore, ORD should consider an investigation into so-called "risk-risk" trade-offs affecting the various types of costs and burdens imposed on households as a result of environmental controls. Potential cost savings of $40 billion from greater use of incentive-based regulation, as has been suggested, may be one of the most effective programs for improving public health and the environment, according to the risk literature, and this is a topic that deserves formal study.

h) The implications of technological innovation in the design of environmental policy were identified by the Committee as a cross-cutting theme in environmental economics research. This topic also deserves special attention in the study of MMI. Important questions remain about the design of MMI policies to promote efficient innovation and technological diffusion. Although there has been important recent work in this area, evolving methodologies make this area fruitful for additional empirical and theoretical study.

i) Developing methods to estimate the cost savings associated with MMI would be valuable for evaluating the benefits of these programs. Estimating these savings is actually very difficult to do. The process requires modeling of costs under both the market approach and a "traditional" policy with the same environmental goal. While a number of prospective studies of the benefits of market mechanisms compared to traditional approaches exist, there have been few retrospective studies of the benefits of MMI.

2.5.2 Research Feasibility

Given the implementation strategy laid out in the EERS; --To what extent is this research scientifically feasible at a high level of quality? --How successful is this research likely to be in answering policy-relevant questions for EPA within the next 8-10 years?

Environmental economic researchers have examined many of these issues at some level, although further work needs to be conducted. It is very likely that high quality and highly policy-relevant work can be conducted in the next 8-10 years.

2.6 Benefits of Environmental Information Disclosure

2.6.1 Characterization of Research Gaps and Priorities and Research Feasibility

Is the characterization of each of the major research gaps in the literature adequate? Will these priorities and implementation approaches effectively address the areas of greatest scientific uncertainty?

Given the implementation strategy laid out in the EERS; --To what extent is this research scientifically feasible at a high level of quality? --How successful is this research likely to be in answering policy-relevant questions for EPA within the next 8-10 years?

Several statutes under which EPA operates—including the Emergency Planning and Community Right-to-Know Act of 1986 and the Safe Drinking Water Act amendments of 1996—require that information about environmental performance be disclosed to affected communities and/or the general public. EPA states that there is no generally accepted method to estimate the benefits of such information disclosure, although selected anecdotal evidence suggests that information disclosure programs may affect the behavior of entities that are required to provide the information.

EPA is correct that there is no generally accepted method, but it is not clear that one should search for a "general method." The fact remains, however, that research to date has not estimated the benefits (or the costs) of environmental information disclosure programs. For the most part, the major analytical challenge is not associated with monetizing impacts, but with identifying behavioral responses to information disclosure requirements.

Clearly, there is increasing interest in the United States (and other countries) in the potential role that can be played by information disclosure programs, as substitutes or complements for conventional command-and-control or market-based environmental policy instruments. Much of this interest can be attributed to the success that has been claimed for the Toxics Release Inventory (TRI) program, which requires large manufacturing facilities to report publicly their annual releases of certain chemicals. Since the inception of the TRI program in 1986, reported releases of over 300 regulated chemicals have fallen by more than 45%.

What is needed is analysis of the efficacy of such information disclosure programs by examining the ways in which these programs can–in theory–affect environmental quality and by investigating empirically the ways in which the programs have actually affected pollutant releases. In terms of theory, there are several pathways through which information disclosure might lead to pollution reduction, including: green consumerism, green investing, community pressure, impacts on labor, the threat of future regulation, and organizational limitations of the firm. Better theoretical modeling of firms' production and pollution decisions would incorporate these pathways. Such theoretical frameworks could then be used as the bases for empirical (econometric) analyses of the effects of TRI on facility decision-making. Such research could produce greater understanding of how facilities respond to information disclosure programs such as the Toxics Release Inventory. This is a necessary first step to estimating the benefits of such information disclosure programs.

As EPA notes, there are a variety of other important research questions, including cost-effectiveness comparisons of information disclosure programs with command-and-control and/or market-based environmental policy instruments. In general, EPA's Office of Research and Development has begun funding such research efforts under its category of "Corporate Environmental Behavior: Examining the Effectiveness of Government Interventions and Voluntary Initiatives," part of the Science to Achieve Results (STAR) program.

It is important to distinguish between information disclosure as a complement and as a substitute for other forms of regulation. EPA suggests in places that information disclosure might be a substitute for regulation (as in the third bullet point on page 20). While disclosure requirements might induce pollution reductions, theory suggests that only in a limiting case would these reductions match those under efficiency-maximizing regulation. In the context of the TRI, firms would need to assume that, at the margin, the public-relations cost of pollutant emissions (that is, the negative impact on product demand) were equal to marginal environmental damages. This would only be the case if customers fully internalized the costs of pollution in their purchasing decisions. It seems more likely that information-disclosure requirements will lead only to partial reductions in pollution, relative to the efficient level of reduction.

This suggests that information-disclosure requirements may indeed be a substitute for regulation that is *less* stringent than the efficient amount, but that they are unlikely on their own to yield large enough reductions to correspond to the *efficient* level of pollution-abatement. This also suggests the need for research that examines how information-

disclosure rules and other regulatory approaches will operate jointly. Even when the two instruments — information disclosure and other regulation — are used together, there are potential cost-savings from the information-disclosure component, for example by facilitating monitoring and enforcement activities.

These considerations imply several additional research questions. First, how large is the reduction in pollution induced by information-disclosure requirements, relative to the efficiency-maximizing reduction? Correspondingly, what fraction of the damage from pollution is internalized in the purchasing decisions of consumers, once they are aware of respective pollution? What are the cost-savings from combining information-disclosure requirements with direct regulation, compared with costs under direct regulation alone?

2.6.2 Usefulness of the Research

What improvements in the design and implementation of the EERS would make each research project more useful to EPA and other environmental management agencies?

The topic of information disclosure relates closely to another potential area of economic research for EPA that is likely to become more important over time, with increasing concerns about the effectiveness of terrorism risk policies on plant safety and security. In particular, EPA might support econometric analysis of the effectiveness of current and proposed terrorism risk regulations on plant safety and security, drawing upon publicly available data, as well as the Risk Management Plan database housed at EPA's Chemical Emergency Preparedness and Prevention Office, and data collected by local emergency preparedness agencies under the Emergency Planning and Community-Right-to-Know Act.

As noted above, several regulations, beginning in the late 1980s, were designed to reduce the risk of large-scale chemical accidents. The Emergency Planning and Community-Right-to-Know Act of 1986 established disclosure requirements for plants using and storing hazardous chemicals on-site. Section 112(r) of the Clean Air Act Amendments of 1990 requires detailed risk management planning and reporting for all large chemical plants. The International Standard Organization (ISO) developed a set of management practices designed to improve environmental performance, but also likely to reduce risk from chemical use. And in the aftermath of September 11, 2001, the American Chemistry Council, a trade organization representing the largest chemical manufacturing firms, established management practices for enhancing the security of chemical plants.

While most of these programs and policies were not specifically designed to reduce risk from terrorism, studies of the effectiveness of these approaches will provide valuable information regarding the viability of alternative types of policies to reduce the environmental component of terrorism risk. Until now, little empirical research has been conducted to evaluate the relative effectiveness of these regulations on plant and community safety.

3. ANSWER TO CHARGE QUESTION 3

3.1 Missing Issues

Can the SAB identify by consensus any environmental economics issues of overriding importance to EPA that the EERS has missed, and that EPA should address provided that more resources be made available for Environmental Economics Research?

Within each of the five subject areas discussed in section 2. of this report the Committee has identified topics not originally mentioned in the Research Strategy. For example, under market methods and incentives we have suggested that the agency broaden its purview to consider renewable portfolio standards and incentives other than permit trading. In the case of valuation of environmental benefits we have stressed the importance of valuing non-marginal as well as marginal changes in risk of death and threats to ecosystems. The EEAC, however, believes that that the five subject areas on which the Agency has focused in the EERS are the most important areas of environmental economics research in terms of their importance to the Agency and in terms of gaps in the literature.

There is, however, an additional area of research, namely, the distributional consequences of regulation, to which the Agency might give more attention. We know that it is difficult to attribute the full general equilibrium costs and benefits of any given policy to specific groups of individuals; however, as long as people have only one vote each, and cannot exercise a number of votes in proportion to their perceived individual net benefits from environmental regulations, an awareness of distributional consequences will be important to the political feasibility of environmental regulations. Even a very attractive potential Pareto improvement will not fly if the distributional consequences do not meet with society's approval. The political economy of regulation will be an enduring dimension of successful environmental management. We urge the agency to give more weight to the distributional consequences of regulation in its strategy for benefit valuation. And, to the extent possible, in the measurement of the costs of environmental regulation.

4. ANSWER TO CHARGE QUESTION 4

4.1 Communication of the Research Strategy

What is the best way for EPA to communicate the results of the research strategy and plans for achieving its long-term research goals to the wider research community, and other potential users?

One way in which EPA could foster dialog with members of the research community is to hold workshops in conjunction with the American Economics Association and American Agricultural Economics Association annual meetings. These sessions would both give the Agency an opportunity to communicate its long-range research goals and to hear from researchers how these goals might best be met. This would inform the Agency's formulation of Requests for Proposals.

Currently, EPA's requests for proposals are announced 90-120 days in advance, and may be found on EPA's website under "Funding Opportunities—Environmental Research Grant Announcements." [http://es.epa.gov/ncer/rfa/] The EEAC suggests that these notices also be sent to the Chairs of Departments of Economics and Agricultural Economics, and that announcements be placed in the newsletters of the Association of Environmental and Resource Economists and American Agricultural Economics Association.

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