NC STATE UNIVERSITY

May 16,2002

Dr. John Morrall
Office of Information and Regulatory Affairs
Office of Management and Budget
Executive Office of the President
725 17th Street, NW
Washington, D.C. 20503

Dear John:

I am writing in response to your e-mail sending OMB's draft annual report to Congress on the Cost and Benefits of Federal Regulations. The report requests comments on analytic issues associated with the guidance provided on benefit-cost methods. My comments are selective, addressing some of the issues under review by the OMB/CEA task force as well as other analytical issues that are not identified in the report.

(1) <u>Discount Rate</u>

The current **7%** real discount rate is too high. A lower rate is clearly warranted. **A** reasonably current and accessible discussion of the conceptual issues is available in the Portney-Weyant volume published by RFF in 1999. **A** key distinction raised in their volume is very long term, or what is labeled "deep future," that almost necessarily involves significant uncertainty. Use of the present value criterion for those cases is a key part of the issue. I argued in my chapter in this volume that these types of decisions are more profitably translated into choices about insurance for the future. However, I feel that serious consideration should be given by the OMB/CEA group to all the positions advanced in each of the chapters and it may be prudent to consider a different discount rate plus supplementary information for these deep future policy choices. For practical purposes, with most regulatory decisions I feel periodic review of the discount rate is warranted with comparisons made to a real rate of return that is a weighted average following the early Krutilla-Echstein logic that attempts to take account (in a very approximate way) of the types of expenditures that public investments versus regulations displace. This means periodic review of any specified rate is warranted. The criteria for change should be identified and the time horizon over which reviews are made needs to be stated. Computations of the required real rate of return should be transparent and changes should be smooth (i.e., a bound placed on the size of adjustments if they are made every three to five years)

(2) Latency Period

   A clear distinction must be made between the "cessation lag" and the "latency
   period." The report of the EPA/SAB Arsenic Rule Committee (and especially
   discussion by Dr. Maureen Cropper) made this argument clearly (a copy is attached).
   What is at issue is the distinction between two types of lags. The first arises because
   of the delay associated with the time period when exposure takes place and the health
   effect occurs. This time span is usually labeled the latency period. There is a second
   type of time interval that relates to the time between a policy to reduce exposure and
   when the resulting profile of exposures people receive changes sufficiently for some
   modification in the effects of the regulated pollutant to be observed. This is the
   cessation lag. It is affected by the implementation process, the exposure process, and
   the latency period.

   Both time lags affect the evaluation of policies involving risk, and thus require clear
   guidance on the modeling issues. The discussion Dr. Cropper prepared for the
   arsenic report develops the basic reasons why they are important so I will not repeat it
   here.

(3) Quality Adjusted Life Years

   People care about the quality of their lives as much, or more than, the risks they face
   of premature death. It is extremely important to consider specific quantitative
   adjustments for policies that alter the risks of death people face.

   The issue raised by the QALY approach highlights two aspects of the object of choice
   in these policy evaluations – the probability of living and the living conditions facing
   people. The latter poses an index number problem comparable at the conceptual level
   to quality adjustment issues with ordinary commodities. If a car or a computer is
   improved we like to measure the "amount" of each good in standardized units. One
   reason for this effort arises because we want to distinguish the source of price
   changes. If the price increases we generally like to separate amount of the good
   received from the increase in cost of producing the same good.

   There is a great deal of complex theory and practice in this literature (see Bresnahan
   and Gordon's NBER volume [1997] for a summary of some of the issues). Pakes'
   recent NBER working paper #W8715 entitled **"A** Reconsideration of Hedonic Price
   Indexes with an Application to PC's" [Jan. 20021 discusses concerns about using
   hedonic models for price indexes. There is controversy with his arguments as well. I
   cite it here because it reflects the importance of the details in each case.

   Fundamentally, we need to measure how people evaluate the distinctions in the
   amount of the good due to quality change. The best overall summary of the analytical
   issues in my judgment remains Willig's paper in the *Journal of Economic Theory*
   [1978].

Quality of life adjustments raise substantial issues in recovering the information that is needed about how people evaluate the equivalency of different restrictions on their activities of daily living and the role of direct experience versus indirect conceptualization of "what it would be like" to experience specific conditions. There is insufficient information in the literature to recommend specific guidance here. In my opinion, we know it matters a great deal <u>and</u> adjustment is warranted. What we don't know is how to distinguish unobserved heterogeneity in how different people deal with these limitations in order to construct a suitable "*average*."

With market goods, arbitrage establishes a tradeoff between quality and quantity based on this heterogeneity in beliefs and ability to pay. We don't have the equivalent for restrictions to activities of daily living versus life years.

<u>QALY's do not provide the answer</u>. They are defined based on a very specific tradeoff, usually a specific algebraic treatment of preferences (in a simple standard gamble format) and fail to consistently treat the quality/quantity tradeoff They cannot be treated as independent of the conceptual modeling underlying conventional VSL estimates.

What is needed is a modest review of the literature, a few targeted studies using conjoint methods, and a framework that incorporates the quality/quantity tradeoff into a consistent treatment of risk. Work along these lines has started in a number of areas of the literature. The review would distill it with a specific focus. This cannot be summarized in a brief set of notes and is a separate research task that should be done and presented to the OMB/CEA group. I would be happy to provide references and the names of people working in this area.

(4) <u>Risk Assessment for Special Groups</u>

I believe that risk assessment and micro-epidemiology of the environmental risks faced by specific groups are extremely important research topics. I was amazed to learn (and it may be that I simply missed some literature) that there appear to have been no micro level studies evaluating the link between the outset of health conditions (e.g., lung and heart related) and exposure to air pollution. We routinely use mortality studies to assess the risks of exposure to specific air pollutants as key information in establishing standards for the criteria air pollutants. These studies rely on causal mechanisms that do not appear to have been evaluated. That is, the micro-epidemiology literature does not appear to have considered using panel data that would track the onset of these conditions.

(5) <u>Ability to Pay and Willingness to Pay (WTP)</u>

An issue not identified in your list and one of special importance (in my opinion) is the assumption that unit benefits associated with changes in environmental services, health effects, risk or any outcome linked to the environmental policy of interest are treated as constants. That is, current practice in benefit-cost analysis treats each

project or policy and the effects of each – even within a given policy – as independent from the perspective of valuation. Thus, the unit values associated with avoided asthma attacks are independent of the unit values for bronchitis. The need to "pay for" both of these changes is never considered. The prospect of overlap is sometimes discussed but not seriously addressed. That is, at the end of the day WTP calculations must be consistent with and reconciled to a given level of household (or representative agent's) income. When we conduct policy analyses that act as if people are "paying" for proposed changes we must evaluate payments for an interrelated set of effects with unit values that recognize that all payments are assumed to be made. That is, they all would be subtracted from income and as a consequence affect the unit payment a person would make for any one effect.

Current practice is a legacy of the "small in relation to the aggregate economy" assumption of current benefit-cost methods. However, both EO 12291 and 12866 require a focus on significant policy interventions. Thus, we select regulations to evaluate that have greater potential for invalidating the assumptions underlying our benefit estimation methods.

This simple point (i.e., that WTP must be constrained by ability to pay) is missing from the EPA Retrospective and the Prospective analyses and nearly every other RIA I have examined over the past 25 years.

This is one of the reasons why the benefit estimates derived in the Retrospective and Prospective seemed so large. No one considered (until after the fact) using the relation to income as a cross-check. It was the first signal to some of us that the Costanza et al., 1997 *Nature* paper costing or valuing the earth's ecosystems was so wrong (see Bockstael et al., [2000], enclosed).

There are methods to build in this type of check. I have included a reprint of one of them that I helped to develop. The check of estimated "payments for a policy" at the household level in comparison to income would be a great plausibility check even if the methods do not explicitly "build in" the restriction.

I urge that serious consideration be given to adding this point to the list of analytic issues that are considered.

I appreciate the opportunity to comment and hope these suggestions are of some use.

With best regards,

V. Kerry Smith
University Distinguished Professor