Comments on MS 576 "Climate Risks and Carbon Prices: Revising the Social Cost of Carbon"

The paper "Climate Risks and Carbon Prices: Revising the Social Cost of Carbon" reviews the work of a U.S. Government interagency working group that produced estimates of the social cost of carbon (SCC) and presents alternative SCC estimates. After critiquing various elements of the U.S. Government analysis, this paper constructs SCC estimates based on alternative assumptions regarding climate sensitivity, damage functions, and discount rates with the DICE model. This analysis yields some estimates of the SCC in the hundreds of dollars per metric ton CO2. This review provides four major comments on the manuscript.

(1) The SCC represents the marginal damage of an incremental ton of CO2 emissions. The social cost of carbon will vary with large changes in the projected trajectory of CO2 emissions. Thus, the social cost of carbon over the course of this century will differ significantly between a "business-as-usual" scenario and a reduction to zero emissions scenario, as described at the bottom of page 2. It is not appropriate then to compare SCC estimates from "business-as-usual" scenarios with the marginal abatement costs of zero or negative emissions scenarios to identify the socially optimal policy, which is the basis for the claim in the second paragraph of the abstract. Stern (2006) explicitly notes that the SCC falls by about two-thirds going from a BAU to a concentration stabilization scenario in his analysis.

(2) The alternative damage functions are problematic. I have several reservations regarding the use of modified damage functions based on the Hanemann and Weitzman working papers. First, these papers have not been subject to peer review, so it raises questions whether they are ripe for use as inputs in a paper seeking publication in a refereed journal. Second, the Hanemann analysis does not produce an alternative damage function. It simply produces a different point estimate of U.S. climate change damages with 2.5C warming than Nordhaus and Boyer (2000). Thus, it is a significant reach to assume that this point estimate for the United States can simply be extended globally and over a range of temperatures as represented by the function in equation (3). It is more than a reach, however, it is incorrect. The Hanemann paper compares his analysis to the Nordhaus and Boyer (2000) damage function for the United States at 2.5C. Nordhaus has since updated the damage function, which generates nearly double the global damages for 2.5C warming than the Nordhaus and Boyer damage function (see Nordhaus 2008 figure 3-3). Thus, a factor of four adjustment to DICE-2007 is incorrect based on a comparison of Hanemann and RICE-1999. Third, the Weitzman paper merely posits a damage function adjustment for large temperature increases of 6C and 12C based on a casual characterization of the planet the last time global temperatures were at or near these levels. Weitzman does not build up from an empirical scientific or economic assessment of potential impacts his damage function, as is the norm in the SCC literature, and for that matter, the norm in the entire environmental economics literature on the social damages caused by externalities. Weitzman is speculating about the economic magnitude of catastrophic climate change.

(3) Should a national or global measure of the SCC be used by the USG? This paper modifies several inputs to the DICE model to generate new estimates of the SCC. All modifications appear intended to increase the value of the SCC. (Indeed, this appears

to be the objective of the paper given the first paragraph. What is the basis for the statement in the final sentence of this paragraph – that the USG SCC estimate is "difficult to reconcile with the belief that it is urgent to take action…"? What is the basis for this "belief"? The IPCC can't be the basis for this, which is cited in a footnote, since the IPCC does not prescribe policy objectives. It gives the impression that the paper begins with a bias against the USG estimate on non-economic grounds, which strikes one as odd for the journal Economics.) A key assumption made by the USG is to use a global measure in its analysis of domestic regulations. This is a strong and very rare assumption (e.g., the USG does not include the Canadian benefits of reducing SO2 emissions from US coal-fired power plants in its regulatory policy evaluations). A credible evaluation and extension of the USG analysis should also probe the economic significance of this global assumption by explicitly exploring a domestic-only SCC measure.

(4) What is the basis for comparing a handful of new SCC estimates with the distribution based on thousands of model runs by the USG? The premise of this paper appears to be that the central estimate of the USG SCC exercise is too low. This paper then produces 16 SCC estimates as evidence that the SCC could be much higher than the primary estimate of $21/tCO2. This is already evident in the USG SCC exercise, which presents some estimates for the SCC in the hundreds of dollars per ton CO2 as well (see appendix in Interagency Working Group on SCC 2010). To add value to this literature and provide useful insights on the USG work, this analysis should undertake a careful evaluation of uncertainty. Without a formal representation of the probabilities for the states of the world implicit in the 16 estimates in this paper, it is impossible to assess the value of this analysis. If a case can be made that these 16 estimates span the space of potential states of the world with unabated climate change, then they can be used to construct a SCC point estimate (as well as a distribution) for use in policy making. If they represent only a small fraction of the potential states of the world, then they paint a very incomplete picture and are not sufficient for use in benefit-cost analysis of proposed climate change policies.