

Co-editor's evaluation of *Economics* discussion paper 2012-37, "The Rate of Change of the Social Cost of Carbon and the Social Planner's Hotelling Rule," by Tomas Kögel

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In his response to the reviewers, the author suggests that this manuscript -- which was submitted as an entirely new paper -- should be subject to a circumscribed review process because it is a complete rewrite of a paper that was previously rejected with encouragement to provide a new submission. This is predicated on a misunderstanding of the process; a new manuscript is a new manuscript, and is subject to a full review process. The new manuscript was requested because the level of revision required to the predecessor manuscript was sufficiently comprehensive that a full review process was needed.

I have received three reviews of this (new) manuscript. I regret that none of the reviewers overlap with the set of reviewers of the authors' previous manuscripts, but this was not for want of trying; and my suspicion is that this mix of reviews is more favorable than would have been attained had the previous reviewers been willing to review this new manuscript.

Of the three reviewers, the first suggests that the paper provides a solid review of the relevant literature but expresses some confusion about the relationship between the polluting resources and its released pollution content. The second suggests that the paper is largely standard and not novel, finds the paper unclear, and, like the first, expresses some confusion about the relationship between the polluting resources and its released pollution content. The third is largely positive, but again expresses some concern regarding the description of the relationship between carbon emissions and fossil fuel use.

Two others provided short comments on this manuscript. One calls it an excellent contribution, recommends approval, and suggests an exploration of the diversity of climate damages. A second commends the paper as providing a sensible but standard model, and suggests that it provides no interesting extensions to the standard model

I agree with the referee who writes that this paper provides a good and necessary synthesis of views on the rate of change of the SCC. With some general tightening of the language and the addition of a clarifying set of numerical examples, it would be well worthy of publication. I therefore recommend publication provided such revisions are made.

In my evaluation of the previous iteration of this manuscript, I wrote, "In light of my recommendations on the original manuscript, as well as the comments by both reviewers on the new manuscript, I am unable to recommend publication at this time. Both reviewers' comments and my original editorial report point to the need for a stronger discussion section, including a numerical illustration of the results; reviewer #2's comments and the original editorial report also point to the for the new work to be better placed in the context of existing literature. A paper addressing both these concerns might be worthy of publication, but would necessarily constitute a new submission."

This echoed a statement in the evaluation of the preceding version, “that this paper gets a little lost in abstraction -- a trait which, as reviewer #1 observed, was even more true in the original version of the manuscript, which focused exclusively on a currently unrealistic steady state economy. I think the paper would benefit significantly from accompanying the mathematical formalism with stronger intuitive development of the model. A numerical example might be particularly useful, as would greater grounding and discussion of modeling choices within the context of the existing SCC literature.”

As is clear from these evaluations, the author is in error when he writes that “my writing style did not bother the referees of the predecessor version [sic] of this paper.”

The author has addressed the need for the work to be placed in the context of the existing literature, but has ignored the need I expressed for “ a stronger discussion section, including a numerical illustration of the results.” The current manuscript continues to get lost in abstraction, which I believe is why all the reviewers -- some largely positive, some more negative overall -- expressed some form of confusion regarding the relationship between the polluting resource and its released pollution content. If the author included corresponding numerical examples, they would necessarily force clarification of some currently obscure issues.

As one central example, the author repeatedly refers to the case in which “a unit of fossil fuel use leads to exactly one unit of carbon emission.” This is an ungrounded abstraction: a “unit of fossil fuel use” and a “unit of carbon emissions” are not things that exist apart from a particular pair of unit definitions. How can the rate of growth of the SCC depend on whether fossil fuels are measured in terms of Gt C or tonnes oil equivalent? Clearly, they cannot, and a numerical example would force the author to clarify what is meant by this core concept. It would more generally be valuable for communications if the author described more clearly why alternative forms of the relationship between fossil fuel use and carbon emissions held.

As an minor aside, I know that it is in error to assert that IAMs employ constant discount rates; they generally employ Ramsey discounting, and whether this yields an approximately constant discount rate depends upon the particular scenario under consideration.

As another minor aside, the author has placed this manuscript in the context of relevant literature -- but I was quite surprised that the relevant literature consisted of an almost non-overlapping set with the IAM SCC literature that I had originally had in mind, and which is represented by every other submission to the special issue. This is not the author's fault, and I do not think needs to be changed, but it does suggest that multiple, almost non-communicating groups of researchers are working on the SCC. I leave it to the author's judgment, but it would help in tackling this divide and in providing a clarifying numerical example if he were to consider how his model applies to the Anthoff et al. discussion paper (MS 2011-44) in this same special issue, and/or the time series of social cost of carbon values in the U.S. government's 2010 social cost of carbon analysis.