Reply to the referees' reports on dp 2012-37 ("The Rate of Change of the Social Cost of Carbon and the Social Planner's Hotelling Rule" by Tomas Kögel)

Before I go into the details, I would like to thank the referees for their reports.

1. General remark

To put the present version of this paper better into its context, it seems useful to explain the history of the paper. The predecessor version of this paper, which I had on 11 January 2012 uploaded on www.economics-ejournal.org/economics/discussionpapers/2011-35/view, was arguably - with certain qualifications that are explained in section 5 of this paper - the literature's first paper that derived the growth rate of the social cost of carbon (SCC). For simplicity, I assumed in that paper the following one-to-one-relation between carbon emissions and fossil fuel use:

$$M = R, \tag{1}$$

where M denotes the flow of carbon emissions and R denotes the amount of fossil fuel use. The first referee and the co-editor of that paper accepted that paper for publication in this journal, subject to that the second referee of that paper will not find a major flaw in the paper. The second referee of that paper however argued that the paper would not be a contribution to the literature. According to my reading of his report, he came to this view because in his view the growth rate of the SCC would not differ from what has been derived in Groth and Schou (2007, eq. (49)) and in van der Ploeg and Withagen (2011, eq. (7) and eq. (8)). Those equations were however the Hotelling rule (and in case of eq. (7) in van der Ploeg and Withagen, 2011, a rule determining the rate of change of the Hotelling rent). Nevertheless, the determinants of the Hotelling rule in those papers and the determinants of the growth rate of the SCC in my paper were, by and large, identical because those papers and my paper assumed eq. (1) to hold. In response to the report of the second referee, the co-editor decided that I should place my paper better into the context of existing literature and that this would necessarily constitute a new submission to this journal.

In the meantime, I recognised that the identity of the growth rate of the SCC with the Hotelling rule only holds in the special case in which eq. (1) holds. Moreover, I recognised that the textbook of Perman et al. (2003, Ch. 16) assumes instead of eq. (1) that M=M(R) holds. Therefore, I followed the advise of the co-editor, by assuming in this revision that M=M(R) rather than eq. (1) holds. This allowed me to show that the identity of the growth rate of the SCC with the Hotelling rule only holds true in the special case in which the above-stated eq. (1) holds and cannot be taken for granted.

The above-mentioned correspondence of the referees and the co-editor of the predecessor version of this paper had not been uploaded on the discussion paper website of this journal (I signaled it however in the footnote on page 1 of the present paper). However, in light of this correspondence and in light of the fact that the correspondence took place for this journal, I believe a fair assessment of my paper would require a judgment on whether I acceptably accomplished the task that was assigned to me by the co-editor of the predecessor version of this paper and not so much whether there are further objections that did not bother the referees of the predecessor version of this paper.

2. On the main point(s) of referee 2 and 3

I conducted my above-stated task by replacing the above-stated eq. (1) with the following suggested alternative specifications for M=M(R):

$$\mathrm{M} = \xi \mathrm{R}, \tag{2}$$

$$\mathrm{M} = \mathrm{R}^{\xi}, \tag{3}$$

$$\mathrm{M} = \mathrm{R} - \mathrm{Q} = \mathrm{R} - \mathrm{R}^{\xi}, \tag{4}$$

where ξ is a constant that is possibly lower than one and Q denotes deliberate abatement activity.

Obviously, eq. (2) is more realistic than eq. (1). Moreover, assuming eq. (2) rather than eq. (1) to hold is enough to accomplish my above-stated task to show that the identity of the growth rate of the SCC with the Hotelling rule cannot be taken for granted and only holds in a special case, namely if $\xi = 1$. Of course, referee 2 and 3 did not know the above-mentioned history of the paper. Possibly as a result of this, they objected in their reports that the distinction between eq. (2) and (1) would be very minor. Nevertheless, I can live with their view that the distinction between eq. (2) and (1) is very minor and I can drop this particular distinction from the paper, as the paper allows for the specifications of eq. (3) and (4) as well.

Referee 3 also objects that I do not provide a justification for assuming eq. (3) with $\xi \neq 1$ and argues that one could only come up with a weak justification for it. The reason for that I did not give a justification for eq. (3) with $\xi \neq 1$ is that it represents the most obvious interpretation of M=M(R), where I had taken the latter from Perman et al. (2003, Ch. 16) and Perman et al. also do not give a justification for assuming M=M(R) rather than M=R. In addition, it can be argued that to accomplish the above-stated task assigned to me by the co-editor, the specific reason for why M=M(R) rather than M=R might hold is not so important and all that matters is the argument that it cannot be taken for granted that eq. (1) and its consequences hold. Nevertheless, I can also live with the view that only a weak justification for eq. (3) with $\xi \neq 1$ can be found and I can drop from the paper the distinction between eq. (3) and (1) as well, since the paper still allows for the specification of eq. (4).

Referee 2 finds my eq. (4) to be relevant and interesting, but objects that I discuss a justification for it with deliberate abatement activity only in a footnote and that I only treat eq. (4) as exogenous rather than as endogenous. Again, the objected issues arose from the fact that resolving those issues was in my view not necessary to accomplish the task that was assigned to me by the co-editor. Nevertheless, I can also live with the view of referee 2. As a consequence, I can modify my paper by exclusively focusing on a distinction between eq. (4) and eq. (2) and treating eq. (4) explicitly in deriving the Hotelling rule. The result would be that the growth rate of the SCC and the Hotelling rule, respectively a Pigovian tax on fossil fuel use and a Pigovian tax on carbon emissions, are equivalent if eq. (2) holds, while they are not equivalent if eq. (4) holds.

As I can mention in a revision, in the literature there are proponents of the specification of eq. (2) as well as of the specification of eq. (4) and both groups of proponents provide plausible justifications for their preference. Sinn (2008) would favour eq. (2). This is so because he argues that there exist no feasible technical devices to decouple carbon emission from burning fossil fuels. He acknowledges that so-called ‘sequestration’ or ‘carbon capture and storage’ and ‘afforestation’ would be useful options, but is skeptical towards its practical relevance. In contrast, many researchers, in particular Grimaud et al. (2009) and Gerlagh et al. (2009), would favour eq. (4) because they believe that “carbon capture and storage” is feasible and/or that Q in eq. (4) can be interpreted as a renewable resources sector that is not very fossil fuel-intensive and uses other inputs as well. Moreover, Goulder and Mathai (2000) would favour eq. (4) because they believe in induced technical change that allows for carbon emission abatement. Nevertheless, I believe that an attempt to rebut the view of Sinn lies outside of the scope of this paper. For the scope of this paper, it should be enough to notice that both groups of proponents provide plausible arguments for their preference. If Sinn is right, then the growth rate of the SCC and the Hotelling rule are equivalent, while otherwise they are not.

3. On the main point of referee 1

Referee 1 objects that my paper only derives the rate of change of the SCC as a function of endogenous variables rather than of exogenous variables. Therefore, I do not address what happens with the carbon tax if for example the elasticity of output with respect to temperature rises. My first response is that this dependence on only endogenous variables did not bother the referees of the predecessor version of this paper. Deriving the rate of change of the SCC as a function of exogenous variables lies outside of the task that was assigned to me by the co-editor of the predecessor version of this paper. My second response is that in a general-equilibrium model it cannot be avoided that the SCC depends on the endogenous social discount rate. Moreover, while in a partial equilibrium model the social discount rate is exogenous, it still cannot be avoided that the rate of change of the SCC also depends on the endogenous level of the SCC. I can however add an appendix to the paper in which I derive in a partial-equilibrium version of my model that an increase of the elasticity of damages with respect to pollution increases the Pigovian carbon tax, as ‘requested’ by the referee.

4. On ‘minor’ objections

(i) Referee 1 objects that my paper’s climate change model is just a standard - though sensible - model and that my paper would conduct standard exercises. No matter whether this is really true, I can live with the view of referee 2 that my paper provides a good synthesis of the main contributions on the rate of change of the SCC, which “was probably necessary and should prove to be extremely useful to many researchers” (first paragraph of the report of referee 2).

(ii) Referee 2 objects that I identify the social discount rate with the interest rate although it actually would be a weighting device applied to utility measures. I believe that this is a misunderstanding. The applied literature on the SCC really identifies the discount rate with the interest rate. The weighting device applied to utility measures is in this literature and in my paper labelled as the pure rate of time preference.

(iii) Referee 3 objects that the writing and phrasing of my paper is ‘sloppy’ and that my introduction should be ‘cleaned up’. Again my writing style did not bother the referees of the predecessor version of this paper and the long introduction was another response to the co-editor’s advise that I should place my paper better into the context of existing literature. Nevertheless, I can go through the text and can shorten and ‘clean up’ the introduction.

References

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