

Manuscript synopsis:

This paper derives an expression for the social cost of carbon (SCC) in a simplified economic growth model, where CO₂ concentrations affect utility directly, and they also affect the productivity of the economy. In addition, the pollution production function (i.e. how much CO₂ is produced by a unit of fossil fuel use) is kept general, and the effect of this general function on the Hotelling rule for the price of fossil-fuels deduced. The paper discusses the determinants of the growth rate of the SCC and the Hotelling rule, and shows that when the pollution production function is the identity function (i.e. $f(x)=x$), they are the same, but that this does not hold for more general pollution production functions.

The paper then proceeds to a simple analysis of Pigouvian taxation of CO₂ emissions and fossil fuel use, and once again shows that these are both equal to the SCC with an identity pollution production function, but not in the more general case. The final content section of the paper makes some comparisons of the paper's results to existing results in the literature.

Comments

The claim to novelty of this paper hinges substantially on certain functional modifications of results in the existing literature. To assess the value of the paper, we must ask whether these modifications are:

- a) Empirically plausible/useful
- b) Have non-obvious effects on the results already present in the literature

The most important functional change this paper introduces is a general production function that relates how much CO₂ pollution $M(R)$ results from a flow R of fossil fuel extraction. The fact that this function is in general not the identity function in this paper gives rise to most of its deviations from the existing literature. So, does it make sense to treat $M(R)$ as a non-identity function? Let's look at two cases:

1) Suppose the author thinks $M(R)$ is a fully non-linear function, in which case his model achieves substantive deviations from those existing in the literature, e.g. see eq 14 for the Hotelling Rule, which now picks up an additional (third) term which depends on the time derivative of $M_{\{R\}}$. Is this a credible modeling choice? It took me a while to think of any reason why the pollution production function should be nonlinear as no explanation is offered in the paper - a really unforgivable oversight seeing as the entire paper hinges on this modeling choice. Here is one possibility: There is a capacity constraint in the economy with regards to power generation. So for small amounts of fuel extraction you use a less pollution-efficient (i.e. cheaper) set of processes to turn the fuel into power, but as your usage increases, you are forced to use more pollution-efficient (i.e. more expensive) power facilities, so the pollution from a marginal unit of fuel falls. This suggests $M(R)$ might be concave. This is the best story I could manage, but frankly, it is a weak one, and needs some serious fleshing out to be made credible, e.g. by examining data on fuel mixes as a function of usage. What I think really happens is that $M(R)$ is close to linear up until the point it becomes optimal to switch to some non-fossil fuel based power source, e.g. nuclear or renewables. It MUST be linear if the same fuels are being burnt in the same ways -- it represents the emissions factor of the fuel, and this is, BY DEFINITION, simply a multiplicative factor that converts fuel quantities into CO₂ equivalent emissions.

2) The author tries to make a fair amount of hay out of the case where $M(R) = \alpha \cdot R$ for some α not equal 1, and says that even in this case his formulae suggest a substantial difference between the growth rate of the SCC and the Hotelling rule. If this is his claimed contribution, it is a very marginal one - this is a very minor extension to existing models. So the case for the paper really rests on the strength of the story he tells about nonlinear $M(R)$, which as indicated above, does not exist in the current version.

The strongest point of the paper is that the model is very simple and easy to follow, and it's clearly shown how the Hotelling rule and growth rate of SCC arise from the underlying model equations. This is nice to see, but not very original - most of it follows from existing work.

Proposition 2 and Corollary 2 are completely trivial, and really add almost nothing to the paper. I would remove them - especially the corollary. The whole point of the case where $M(R)=R$ is that in this case

there is really no difference the model can pick up between fossil fuels and CO₂, except that one's in the ground and the other is in the air. Should we be surprised that a pigouvian tax on either is equal to the SCC in this case? No - they're the same thing! This is a totally obvious, almost tautological, result.

On presentational and stylistic points:

The paper is, on the whole, quite poorly written. The writing and phrasing is sloppy, and the language is too imprecise, and occasionally illogical, for an academic journal - e.g. 'substantially almost identical', 'fulfillment of all or many of the following conditions'. In addition, the introduction is poor, and fails to tee up the contribution of the paper in a simple way, but rather rambles across several areas of loosely related literature without much focus. Why do we care about the rate of change of SCC (as opposed to its level), and it's relation to the Hotelling rule? This needs to be very clear from the first paragraph - it is not as things stand. Also, there are several places where I felt the author made inaccurate statements about the literature - e.g. "literature assumes the world temperature to be logarithmic in the carbon stock" - false! Radiative forcing is logarithmic in temperature, and the time derivative of temperature depends linearly on radiative forcing.

Overall, if the author were to resubmit (whether here or at another journal) I would encourage him to focus on what separates his work from the existing literature (i.e the curvature of $M(R)$), explain why this is important (if it is), and justify its inclusion very carefully. I would also suggest that he clean up the introduction substantially, and write more carefully and have his language proof-read by a good editor. At the moment, the paper feels like a minor variation on an existing theme, and a slightly sloppy one at that. We need to know if and why this variation is important empirically.