Referee Report on
The Social Costs of Carbon in the Stokey Model (MS 580)
by Tomas Kögel

The paper by Tomas Kögel analyzes the development of the social cost of carbon (SSC) in a modified version of the Stokey (1998) model with stock pollution. It derives analytical expressions for the SSC during two phases of economic development. In a first step, it focuses on an economy which in early phases of its development makes no effort to reduce pollution. After a threshold is passed, damages from the accumulated stock of pollution become high enough to render pollution reduction optimal. It is argued that the pollution stock grows in the first phase at the rate of output growth while it is constant in the second phase.

The author stresses two contributions of his paper: a) the derivation of an explicit expression for the social costs of carbon and their development over time and b) the result that constant social rates of return to capital can be compatible with growing pollution stocks. I have, however, concerns with respect to both results which mainly result from the set-up of the model. I would not consider the Stokey (1998) model to be well suited to analyze the social cost of carbon – a problem which is even aggravated by the (albeit slight) model modification Kögel suggests. Due to these concerns I find the overall contribution to the literature rather limited.

Let me first expand in more detail on the major concerns I indicated above and then turn to some further aspects.

Major concerns

1. Physically unlimited flow of pollution
   In the chosen model specification of Stokey (1998), it is technically feasible for emissions to grow in the long-run at the rate of output growth since, as the author himself states on page 3, "emissions arise as a by-product of output production rather than from burning a fossil fuel input". Yet, the largest part of carbon emissions indeed stems from the burning of fossil fuels, i.e. exhaustible resources. A model that deals with the very long-run (as it is emphasized by the author on the bottom of page 4) should take account of this limitedness of fossil resource stocks which implies that carbon emissions should decline at least in the long run.
   In the present paper emissions grow at the rate of output growth during the so-called quasi steady state and then remain constant over time. Given a limited resource supply, this development is physically impossible (at least if the stock of pollution degenerates over time). Consequently, the model seems not well-specified to represent carbon emissions and thus to derive the social costs of carbon. One would expect the scarcity of the resource to have an impact on the development of the social costs (not necessarily on its general function form but on their level and possibly growth rate). A paper in which this scarcity is neglected cannot provide answers to whether these effects arise and what impact they have. Consequently the paper's results with respect to the SSC have to be treated with caution.

2. Extend and type of the modification of Stokey's model
   The major modification of Stokey's model regards the utility function in which consumption and pollution now enter in logarithmic form. The author justifies this choice as it allows for a growing

pollution stock to be compatible along the optimal path with a constant social return to capital (I come back to this aspect under 3.). I find the choice of this utility function problematic due to its implications for the marginal damages from pollution. It is implicitly assumed that marginal damages decrease in the stock of pollution which seems at odds with the standard literature on climate change.

The implications of this assumption are, however, important for the derived development of the SSC. In case of the logarithmic utility, the marginal rate of substitution between consumption and pollution is constant in the quasi-steady state although pollution and therefore damages rise. Assuming realistically that marginal damages increase in the pollution stock would imply non-stationarity of the marginal rate of substitution and thus different optimal SSC. As the development of the SSC over time is one key result, this would impair the contribution of the paper.

3. Consistency with Kaldor facts

As already stated above, the author justifies the adaptation of logarithmic utility as it allows for the joint optimality of growing pollution and a constant social return to capital. He argues that this is important as on the one hand the present situation is characterized by a growing CO2 stock in the atmosphere and that on the other hand "the assumption of a constant discount rate [i.e. social marginal product of capital] seems well-found, as it is compatible with the stylized facts of modern economic growth". The author argues that in the original Stokey model a growing stock of pollution is accompanied by a decreasing social return to capital which would violate Kaldor's stylized facts.

I find this justification for the model modification not entirely convincing. The crucial question is whether Kaldor's stylized fact refers to the *market* rate of return or the *social* rate of return. The author himself states that the "stylized fact of modern growth of Kaldor … can also be motivated by the fact that historical data show trendless market rates of return".

Assuming for the moment that market rates are indeed the relevant benchmark, one would have to consider the solution of the Stokey model for an unregulated market economy (as the last decades have predominantly been characterized by the absence of climate policy at least on a global scale). A cursory analysis of the resulting market solution seems, however, to show that the market rate of return would also be constant for a growing pollution stock in Stokey's original model. So, in order to be in line with the empirical facts of a constant market rate of return in combination with a growing pollution stock the assumption of logarithmic utility seems to be an unnecessarily strict assumption.

Further aspects

4. The paper should provide a better survey of the related literature. During the last two decades a large number of papers was published in the field of growth and exhaustible resources and also a number of papers dealt with SSC empirically as well as theoretically.
5. Regarding the style in which the paper is written, the author sometimes argues rather technically, e.g. regarding the reasons for assuming logarithmic utility, rather than giving economic intuition for his modeling.
6. Although the concept of a quasi-steady state is important for the paper, the term is never defined. While some readers might be familiar with the general concept from the ecological

economics literature, I would recommend explaining it nevertheless, as it might not be known to others.

7. When specifying the SSC, the author shortly refers to van der Ploeg and Withagen (2011) who derive a similar functional form. It would be helpful if some more information was given with respect to the differences in the results between their paper and the paper at hand.
8. The author differentiates between the utility discount rate ρ and the discount rate r which can be rather confusing. It might be better to refer to r as, e.g., the social marginal product of capital.
9. The paper's wording and notation should be checked, e.g.
    - Page 4: "period 0": As time is continuous, it should rather be referred to points in time.
    - Page 4: "pollution starts to improve": What does that mean?
    - Page 5: Is there a minus missing in (5d)?
    - The variable s denotes the savings rate as well as the variable of integration.
    - Regarding footnote 8, I would recommend referring to a different source rather than lecture notes.