Remarks on

A Model of Eco-Efficiency and Recycling

by Mario Cogoy

The paper contributes to an important field of ecological economics. It provides a model with physical flows between biosphere and economy including the extraction of virgin materials, production, pollution and recycling. All material transforming processes are based on technologies with a certain eco-efficiency. This eco-efficiency depends on knowledge or human capital. The physical processes are translated into economic meaningful production functions. Hence, the economic part of the model operates with capital, labor and human capital in a similar fashion as in growth theory. The model is stock-flow consistent and presumes a stationary state. A social planner maximizies the utility with respect to the technology and stationarity constraints.

The portrait of the physical stocks and flows (as summarized in figure 1) is convincing and provides – compared to other models from the literature which is extensively reviewed in the introduction – a rich and interesting structure. One challenge is to relate the physical transformations into economic meaningful processes. This is done by "shape" coefficients which translate mass measures into economic measures. Although this is very simple, it is reasonable and sufficient for the purpose of the paper. Since the relation between economy and biosphere is regulated by the absolute level of the stocks, the eco-efficiency, and the recycling activities, and since there are different trade-offs between the economic decisions reagarding these issues, the model is ambitious and complex. I agree with the author's response to the previous referees that the purpose of the paper is only to study the model structure and the structural role of the mass conservation constraint, eco-efficiency and recycling. At the present state of research it doesn't make sense to overload the model with additional issues from the referee's wish list which makes the model framework much more complicated.

Similar to another referee it was also my first impression that the economic part of the model is less developped: there are no maximizing firms and housholds, no markets for goods, labor and capital, hence there are no prices, interest rates etc. But at the second sight I recognized that these things are not neccessary for the purpose of the paper, namely to study the strucural role of mass-conservation, eco-efficiency and recycling *from the viewpoint of a social planner* (therefore, the technology could also descibe a non-market economy).

I think the paper has a good quality which could perhaps improved slightly by accounting for the following remarks:

- I agree with the author that in a first step the analysis should confine to a comparative-static perspective. Hence we have equilibrium stocks where the flows are consistent with the mass conservation principle. The social planner maximizes per-capita welfare in a stationary state which seems to be a prerequisite for a sustainable economy (see e.g. the first statement in section 6). This could be criticized:

  (a) The underlying concept of sustainablity is not clear. In a dynamic biosphere which evolves in historical time, there is a need for a more sophisticated *dynamic concept of sustainability.* Even without anthropogenetic influence we

have phenomena like irreversible succession of ecosystems, irregular fluctuations of carbon dioxide on a large scale, structural changes in the genetic pool etc. Therefore, it might be not too meaningful for sustainability to preserve a material stock V on a stationary level. For a social planner in a dynamic world who takes the requirements of sustanability into consideration it might be sufficient to account for the transversality condition $\lim_{t\to\infty} V_t \geq 0$ in a dynamic optimization program. Also in a dynamic approach the mass conservation condition has to be met for any time point.

(b) The social planner chooses an optimal level of H (human capital or knowledge) which has to be maintained according to (21). It is a little bit misleading to call this an *endogenous* determination of the knowledge level (p.4), since knowledge does not evolve according to the decisions of the agents. There is no R& D, no knowledge generating process, but the social planner "chooses" a knowledge level. This is a little bit strange. Does this imply that curiosity of the agents and spontanous new ideas are bad for welfare and a danger for sustainability?

- Human capital is interpreted as an externality (p.12) since it influenes the efficiency of all transformation processes in a non-rival way. However, this should be related to the term "*knowledge*" rather than "*human capital*" because the latter is *embodied* in individual labor and hence rival in use. For this reason, in most growth models human capital enters the production function as a specific input (like physical capital and labor) and hence human capital has to be allocated to different production sectors (here: H_v, H_p,...). Knowledge, in contrast, is non-rival in use and enters the production function by influencing the total factor productivity, similar to the model presented in this paper. This should be clarified instead of using knowledge and human capital as synonyms.

Some minor remarks:

- The reference to figure 1 should be placed at the beginning of the section. This would alleviate to keep track of the model structure. Without the help of the figure, the reader does not understand the new symbol r in eq. (8).

- Labor input is denoted by l_v, l_R etc. This is not consistent with the convention that all lower case variables are flows and upper case variables are stocks (see eq. (26) where lower case variables add up to an upper case variable; by the way: why N instead of L?).

- The author tends to make some severe simplifications *en passant* and conceed these simplifications (and expound their problems) several pages later. Example: The identical influence of human capital η on all transformation processes, the identical production function in all sectors. If the qualitative results of the analysis depend crucially on these simplifications then they should be critically discussed in more detail and earlier. If they are of minor importance for the qualitative results (this is what I guess), then also this could be mentioned earlier.

- The utility function (29) depends negativly on K and D. It would be helpful to refer to the literature in order to justify these assumptions as they are important for the results.

- The maximization problem (p.16) should be explained in more detail: What are the *decision variables* of the social planner? In the first order conditions I see derivatives to $v, r, \eta, \delta, \pi, K, D$. But not all of them are decision variables since e.g. v depends on D via eq. (5), π and η are connceted via eq. (23) and so on.

- The results of the analysis as discussed in section 6 are interesting and insightful. Especially the "non-intuitive" results (p.28) deserve further explanation. Despite the fact that the paper adopts a comparative-static view, the author should put up for discussion that it is not clear whether there exists a (stable?) path to such an optimal equilibrium.

Summing up, these remarks should not be understood as objections against this original, carefully written and insightful paper, but they might help for further improvement.