



DICE. It then, seemingly, expands the complexity of the considered processes. What is missing is a careful comparison of the new model with the closest approximation (one may assume DICE to be this models) in terms of the number of parameters, the number of equations, the number of decision variables, and the considered processes. Having the code available in an appendix would also simplify the discussion and the ability to reproduce the results.

3. Several assumptions are difficult to understand. For example, why does only governmental spending on abatement affect production possibilities (p. 828. L. 13)?
4. The paper contains several claims that are not substantiated by / easily accessible from the provided evidence. Examples include:
  - a. Motivation of IAMs (P. 822, l 25-27).
  - b. Does (UNFCCC, 1992) really call for a two degree C limit? In which article?
  - c. Is this really a “win-win situation” (p. 843. L. 4). Figure 1 e suggests that current generations may loose something.
5. What is the logic behind the mapping of the 2 degree target to a single atmospheric CO<sub>2</sub> concentration (p. 839)? What about an overshoot?
6. The language needs a careful round of editing to address issues with word choices, grammar, and style.
7. The wording is often ambiguous. For example:
  - a. How is a “best approach” defined (p. 824, l 21)?
  - b. What does it mean when future values are “not know” (p. 824. L2)? Does this not apply to all other projected numbers?
  - c. What does it mean to “enhance the quality of life for all” (p. 843. L. 2) in the framework of this model?

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8. The citations are imprecise. For example, on which chapter and page in “(IPCC, 2013)” should the reader look to see the support for the claims on page 837?
9. What is the relevance of the discussion on the “finite-horizon optimal climate change control solution” (p. 843)?

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Interactive comment on *Earth Syst. Dynam. Discuss.*, 6, 819, 2015.

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