Authors response to interactive comments on **"Coupled Climate–Economy–Biosphere (CoCEB) model – Part 1: Abatement efficacy of low-carbon technologies"** ***by*** **K. B. Z. Ogutu et al.**

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We thank the Referee #1 for her/his comments and respond to her/him herewith. In the following, the referee's comments are in italics, our responses are in Roman, and the changes to be made in the manuscript are in bold. Unless otherwise stated, sections, equations, figures, page numbers, and line numbers referred to are those of the original manuscript.

1. General assessment: The paper presents an IAM with the intent to better explain the role of investments in low-carbon technologies in mitigating climate change. The model is a simplified version of DICE 2013. However, some of the changes made to the economy-components are very counter-intuitive, if not incorrect, and others lack motivation. It is not apparent how the paper improves on previous models and some of the conclusions presented are not supported by the analysis. I recommend to reject the paper.

Our model is mathematically correct and entirely self-consistent; we hope this will be made clear by the following responses. Intuitiveness is, of course, rather subjective. We respect the intuitions of the referee, but we find that our formulation is more intuitive, as explained below. We are open, though, to changes if the editor thinks it is necessary, as long as mathematical self-consistency and correctness are respected.

In formulating the economy component of the model, we are motivated by Greiner and Semmler (2008) who, in turn, extended the DICE framework by including endogenous economic growth and the feedback effects of lower GHG concentrations in the atmosphere on economic growth. The main shortcomings in Greiner and Semmler's (2008) approach, however, are: (i) treating industrial CO_2 emissions as constant over time; and (ii) the inability of their emissions formulation to allow for a total absence of abatement activities (see lines 22–42, p. 1 and line 1, p. 2).

We contend that, in addressing the shortcomings in Greiner and Semmler (2008), this study does actually improve on the literature on long-run economic growth. In fact, because an important stylized fact of market economies is that sustained per capita growth can be observed without a tendency for declining growth rates, it seems necessary to incorporate this aspect in a model dealing with climate change and evaluate climate policies within this framework; see also Greiner and Semmler (2008, p. 57). The main motivation of the modifications with respect to DICE is to make the model simpler and more easily interpretable.

2a. Major comments: The economic part of the model is misspecified. Equation (4) contains the economy-wide budget constraint. It reads

Y- X = I + ME + GE

where X = tau Y is the tax revenue collected by the government and GE is the share of spent on abatement

GE = tauB X = tauB tau Y.

Rearranging yields:

Y = I + ME + GE + X = I + ME + tauB X + X = I + ME + (1 + tauB) X

Total tax revenues are hence not X but (1 + tauB) X.

The referee is very correct about equation (4); the total tax revenue is precisely $\left(1+\tau_{\mathrm{b}}\right)X$. Still, we don't really see why this is a mis-specification: this economic income identity is exactly as in Greiner and Semmler (2008). The government in our economy uses some resources for abatement activities G_{E} that reduce pollution. Abatement activities $G_{\mathrm{E}} \geq 0$ are financed proportionally to tax revenue, i.e., $G_{\mathrm{E}} = \tau_{\mathrm{b}} X = \tau_{\mathrm{b}} \tau Y$. Because $0 \leq \tau_{\mathrm{b}} < 1$, not all of the tax revenue is used for abatement activities. The remaining part of tax revenue, $X = \tau Y$, corresponds to government expenses in such things as education, airports, and postal operations (Mitchel, 2005). These expenses are unaffected by a change of value of τ_{b} , they are not the main focus of the paper and they are held constant. Instead, the focus is on how the fraction $\tau_{\mathrm{b}} X = \tau_{\mathrm{b}} \tau Y$ of the tax revenue is used for abatement activities and what effect this policy action has on climate damages and on GDP growth.

To remove any ambiguity, we rewrite the paragraph on page 5 as:

The paper's main focus is on how the fraction $\tau_{\mathrm{b}} X = \tau_{\mathrm{b}} \tau Y$ of the tax revenue is used for abatement activities, and not on how the government uses its total tax revenue $X = \tau Y$ to generally macro-manage the economy; see, for instance, Mitchel (2005). In fact, our formulation assumes, furthermore, that government spending, except for abatement, is held constant throughout this work, so that only changes in abatement affect the size of per capita GDP (see also Greiner and Semmler, 2008, p. 64). On the one hand […]

We have also added the following reference to the paper's bibliography:

Mitchel, J. D.: The impact of government spending on economic growth, available at: www.heritage.org (last access: 9 March 2017) 2005.

2b. Furthermore, the statement on p. 5 that "government spending, expect for abatement, does not affect the size of per capita GDP" is incorrect. Total output (after damages have been deduced) is allocated over different uses: investment (I), consumption (ME), abatement (GE) and other government spending (X). This directly follows from the budget equation (4). However, it is not at all clear what other government spending is used for – or why it is there at all. It seems that the government is burning the money (but at least without creating additional GHG emissions). Any form of optimization (which is absent from the model) would immediately result in tauB = 1 and a reduction of X to the preferred level of abatement. Currently there is a substantial inefficiency (X) in the model that is not motivated in any form and maybe more worryingly, the abatement effort is directly tied to the size of this inefficiency (GE = tauB X).

Indeed the sentence is not clear enough, and we changed it to: "**government spending, except for abatement, is held constant throughout this work, so that only changes in abatement affect the size of per capita GDP.**" As mentioned in the preceding reply, the non-abatement government expenditure is supposed to go into such things as security, health, education etc. This approach is taken "as is" from Greiner and Semmler (2008, and references therein).

The dependency of abatement expenditure on τ may be disconcerting for the referee; it is exactly the same approach as in Greiner and Semmler (2008), and it appears quite reasonable to the present authors to have abatement expenditures proportional to taxes. Since τ is held constant, the dependence of abatement on the public spending efficiency or lack thereof is only formal: the abatement fraction of tax revenue could be expressed in an entirely equivalent way as proportional to the GDP itself. The sense of the factor $\tau(1+\tau_b)$ that the referee has rightly identified is to express abatement expenditure as a fraction of taxes, or in a more mathematical sense, measure abatement in tax units. For example, a value of $\tau_b = 0.1$ indicates a 10 percent expense for abatement on top of "constant" tax revenue.

Note, moreover, that the growth model presented in this study is not an optimizing model in the generalized equilibrium sense: the consumption, and hence savings, is fixed and is not determined by a decision of the consumers or the firms — i.e. we neglect effects resulting from different preferences (see lines 1–4, p. 5). Therefore, we note that the size and growth of GDP changes as a result of changing the abatement share τ_b. The consequences of neglecting effects resulting from different preferences may partly be addressed by models with overlapping generations of individuals, each of whom lives a finite number of years (e.g., McCandless, 2008, and references therein). Including such savings decisions, however, is again beyond the scope of this study and is left as a worthwhile line of future work.

To clarify matters further, we rewrite the pertinent paragraph on page 5 as:

Note also that the growth model presented in this study is not an optimizing model in the generalized equilibrium sense and we take, as a starting point, the Solow–Swan approach (Solow, 1956; Swan, 1956; Greiner and Semmler, 2008), in which the shares of consumption and savings are given. We do this because we want to focus on effects resulting from climate change, which affect production as modeled in Eqs. (3)–(10) and, therefore, neglect effects resulting from different preferences. The consequences of this assumption may be addressed by models with overlapping generations of individuals, each of whom lives a finite number of years (e.g., McCandless, 2008, and references therein). Including such savings decisions, however, is beyond the scope of this study and is left as a worthwhile line of future work.

We have also added the following reference to the paper's bibliography:

McCandless, G.: The ABCs of RBCs: An Introduction to Dynamic Macroeconomic Models, Harvard University Press, Cambridge, UK, 448 pp., 2008.

3. The growth model itself is highly unusual as well. It uses two stocks of capital, physical and human capital, that are both driven by the same type of investment (I) as in Mankiw et al. (1992). However, although human capital is specified in per capita terms (line 9 on p. 4), its growth rate is linearly increasing in the population growth rate. This would make sense for the total stock of human capital but less so for a per capita measure. Currently it implies that everyone becomes smarter if a child is born in some distant place. Both complications deviate from the DICE model and are not motivated.

We are a little disconcerted by this objection: where exactly is it that the referee perceives per capita human capital increasing with population? In our model, the growth of per capita human and physical capital is governed by equations (10) and (9), respectively. These equations show clearly that H *decreases* with population growth; note the minus sign outside of the parenthesis. Is this just a misunderstanding?

In fact, both of the right-hand-sides of equations (9) and (10) are given by gross investment minus replacement investments. The negative terms on the right-hand-sides of the two equations are also called break-even investments. The latter consists of replacement of worn out capital, furnishing of newborns with capital, and an adjustment due to the continuous upgrading of productivity; see also Eriksson (2013, p. 38).

To clarify things even further, we added the following at the beginning of the paragraph on page 5:

Note that both of the right-hand-sides of equations (9) and (10) are given by gross investment minus replacement investments — these negative terms on the right-hand-sides of the two equations are also called break-even investments. The latter consists of replacement of worn out capital, furnishing of newborns with capital, and an adjustment due to the continuous upgrading of productivity; see also Eriksson (2013, p. 38). Note, furthermore, that the growth model presented in this study is not an optimizing model in the generalized equilibrium sense and we take, as a starting point, the Solow–Swan approach […]

We have also added the following reference to the paper's bibliography:

Eriksson, C.: Economic Growth and the Environment: An Introduction to the Theory, Oxford University Press, Oxford, UK, 234 pp., 2013.

4. The authors see a key contribution of their paper in modelling investments in lowcarbon technologies. Equations (4), (14) and (16) specify the cost and the technology improvement aspects. However, it is not clear how this is an improvement on DICE 2013. The functions are specified somewhat differently, but the underlying logic is the same. The emission reduction rate in DICE is exogenously controlled and differs between scenarios (as does tauB in the present manuscript). Both variables determine both the costs and the trajectory of emission intensity of the economy while actual emissions also depend on output.

We don't consider the way we model investments in low-carbon technologies to be a key contribution, in fact we never say so in the paper. It is true that we consider using a logistic-type function for the technology-dependent carbon intensity of the economy as a more reasonable approach than using an increasing exponential; still, the logic is the same and we never said the opposite. Our key points are reported in the concluding section and the way we model investments in low-carbon technologies is not even mentioned. We don't pretend to improve in this respect the DICE model, just to simplify it. Such simplifications allow us: (i) to carry out an extensive sensitivity analysis in this paper; and (ii) to include further processes in Part 2.

We cannot find anywhere in the text a spot where we imply that investments in low-carbon technologies are a key contribution, and would be happy to reword or delete any phrasing to such an effect, if the referee pointed it out to us. To the contrary, we hope we have further clarified our approach by inserting the following text on page 6:

We now formulate the technology-dependent carbon intensity σ by following the approach of Sahal (1985), who models the replacement of one technology by another using a logistic law. We believe that this approach is more reasonable than the one used in DICE, which uses an exponential law. Our approach is consistent with the point made by Costanza et al. (2007), who argue that the economic growth in DICE is not limited by the availability or non-availability of different energy sources. The energy intensity e_c […]

5. Rather un-intuitively, the authors label D as "damage" (p. 4). However, net per capita output Y is strictly increasing in D. Specifying damages as 1/D would rectify this and would also be in line with DICE. The deviations from the DICE model – at least in the economics part – are either deeply worrying or unmotivated.

This comment is a bit strange: we don't find it less intuitive to have a D or a D^{-1}, as long as either quantity is smaller than unity! The referee seems to agree that the mathematics is correct, though, so we suppose it is just a question of taste. We thought that using the same definition as in Greiner and Semmler (2008) was more understandable, and it *still agrees with the DICE model*. We'll do whatever the editor prefers.

6. I am also concerned about how the authors interpret their results. "As will be shown in Table 3 below, the losses from mitigation in the near future are outweighed by the later gains in averted damage" (p. 10). This statement is not supported by the data presented in Table 3 which contains information on abatement and damage cost for 2100. While for 2100 the reduction in damages exceeds abatement expenditures, the statement contains a comparison between near-term abatement costs and reductions in long-term damages. While not given explicitly, near-term abatement costs are substantial as indicated by the reduction in GDP growth rates (Figure 1e) and the fact that by 2100 total GDP is still highest in BAU (see below). Moreover, such a statement requires to compare costs and benefits across time. Typically IAMs do this by specifying a social discount rate (see the discussion between Nordhaus, Stern and others). The model does not contain a social welfare function or any other indicator as to how costs and benefits should be compared if occurring at different points in time.

"The CoCEB model shows that taking no abatement measures to reduce GHGs leads eventually to a slowdown in economic growth […]. This slowdown implies that future generations will be less able to invest in emissions control or adapt to the detrimental impacts of climate change…" (p. 13) While this might eventually be correct, it is not within the range of data presented in this paper. While the growth rate in BAU is below the other scenarios by 2100, the level of GDP in BAU is still higher than in any other scenario. While these numbers are not reported in the paper, they can be deduced from the numbers on the average annual growth rates given on p. 10 and from Figure 1e. Hence, future generations will be richer under BAU (at least up to 2100) and their ability to invest in mitigation or adaptation is higher not lower than in the other scenarios.

The two statements quoted by the referee are indeed misleading, as they stand, and we thank her/him for pointing these out: we rewrote the first as reported below, and we deleted the second.

In making the first statement, we are referring to figure (1e) and pointing out further supporting evidence from Table 3. We agree with the referee that carrying out a proper cost-and-benefit analysis would require a social-welfare, or optimization, framework. As mentioned already above, such an analysis is beyond the scope of this initial study. Note, however, that recent literature (e.g., Easterlin, 2010; Wilson and Gilbert, 2008; De Neve et al., 2015; Boyce et al., 2016) has argued that — rather than attempting to increase individual and societal incomes — it may be better to avoid income losses, even at the expense of reducing gains, thus giving preference to long-term stability over long-term growth. In the interest of concision, we prefer not to report this debate in the text, hence the choice of deleting the second statement, as suggested by the referee.

In summary, we deleted the lines 17-18 on page 13, and have rewritten the paragraph starting on line 32, p. 10 as:

Since the CoCEB model is not designed for a cost–benefit analysis, it cannot help compare costs and benefits that may occur at different points in time. Doing so would require specifying a social discount rate, as in DICE (Nordhaus, 2007) and PAGE2002 (Stern, 2007). While this is certainly a worthwhile line of future research, it is fraught with considerable danger: the serious divergence between the values assigned to this rate in the work of W. Nordhaus (Nordhaus, 2007) and of N. Stern (Stern, 2007) has led to an acrimonious debate in the IAM literature (Ackerman et al., 2009). Instead, CoCEB does show that the crossover time after which abatement activities start paying off in terms of growth occurs around year 2060. The exact timing of this crossover depends on the definition of damage and on the efficiency of the modeled abatement measures in reducing emissions; see also Bréchet et al. (2015).

We have furthermore added the following references to the Bibliography:

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