Earth Syst. Dynam. Discuss.,
doi:10.5194/esd-2017-25-RC1, 2017

Earth System Dynamics Discussions

Interactive comment on "Combining temperature rate and level perspectives in emission metrics" *by* Borgar Aamaas et al.

Anonymous Referee #1

Received and published: 17 April 2017

A review of "Combining temperature rate and level perspectives in emission metrics" by Aamaas et al. (Earth Syst. Dynam. Discuss., doi:10.5194/esd-2017-25, 2017)

The manuscript proposes a new metric that allows the comparison of different greenhouse gases' climate impacts against that of CO_2. The authors state that the proposed metric combines both temperature change and increase rate impacts of greenhouse gases, using linear weighting of the two components. There has been active research and discussion on climate metrics during past years, and new contributions on the topic might be useful. The manuscript is therefore interesting. The calculations seem to be executed well and the manuscript is pleasant to read.

However, my criticism focuses on the proposed metric itself.

1) There is a major fundamental issue in the proposed rate metric: it is not a rate metric.

Equation (2) doesn't integrate the temperature increase rate $R(t)$, but the temperature level $\Delta T(t)$. The proposed rate metric is therefore identical to the temperature level metric (equation 1), only that the integration limits are based on the years when the rate constraint is binding. It measures temperature level, not the rate, but on years in which the rate constraint is binding. This seems like a strange hybrid to my eye.

The authors mention in section 2 that integrating the increase rate equals the temperature change (i.e. AGTP in the metrics jargon). I believe this has led to the choice of integrating the temperature level and not the increase rate. This argumentation, however, doesn't change the fact that the metric doesn't measure the increase rate. The metric could be renamed, but this would make it less interesting: a variant of the GTP metric with integration over several years instead of a single-year endpoint.

2) There seems to be also a conceptual problem with the proposed metric. The integration ranges are defined to be the years where the chosen baseline scenario exceeds the chosen limit or rate constraints. Because the exceedance is a binary attribute, this definition can make the metric sensitive to the choices over baseline scenario or limits in some cases. The authors discuss this issue to some extent in sections 3.2 and 2.3, and figure 4 shows that the metric does vary considerably between different assumptions on when the constraint is binding. Yet, the general nature of the problem does not become evident from that discussion.

Imagine a scenario where temperature change is stabilized roughly at 2C, but with fluctuations around 2C because it's hard to hit the target spot-on in a dynamic system. This would render the absolute metric calculations of eq. (1) rather arbitrary. This example might be an irrelevant curiosity, but the binary nature of the metric can create problems for a wide number of scenario-limit combinations. Generally, the closer the scenario is on remaining below the limits or exceeding them, the more sensitive the metric will be to small changes in the scenario or the limits.

3) On the practical level, I would also anticipate that agreeing on the baseline scenario

would be a challenge, particularly given the sensitivity noted above. While this is not a flaw of the proposed metric in a scientific sense, it could severely limit its application in practice.

Based on the above arguments, I see the metric as a variant of the GTP with some added complications, which lead to possibly severe problems. It doesn't measure the temperature increase rate, as the label says. Due to the mis-labelling and design flaws, I don't see the metric or the manuscript to be of high quality, and regrettably have to suggest rejecting the manuscript.

Otherwise, there are a number of smaller issues on which the manuscript should be improved:

1) The manuscript resorts to inaccurate argumentation and lax rhetoric in some cases.

First, the authors justify the proposed metrics with Article 2 of the UNFCCC (rows 29-31, 61-62 and 67). I read the article carefully a few times, but I didn't find it mentioning temperature or rates in any way. I assume the authors have made their own interpretations on what the article means. There are no specific temperature goals in the article, unlike is stated on rows 29-31. On row 67 the authors state that the need for the metric is based on article 2. Yes, there might be a need, but it is not based on the article. The article only mentions the stabilization of greenhouse gas concentrations, which could be implemented in absence of climate metrics by setting separate concentration limits for each gas. (The article is mentioned again incorrectly at row 100, by stating that the rate causes damages. Yes is does, but article 2 doesn't state that.)

This argumentation gives a false impression that the main article of the UNFCCC would require a climate metric just like what is proposed in this article (row 397). I disagree strongly.

Second, the rate metric of equation (2) is motivated rather loosely on row 167 with "any additional warming is equally critical throughout the period of the binding rate

constraint". I don't agree with this statement, for marginal changes at higher levels or rates can inflict much higher damages. Also, if the statement were true, wouldn't it make sense to integrate also years other than those on which the limits are binding?

2) There are a number of points where the readability should be improved:

Row 14: Why 'baseline scenario'? Couldn't it be just 'scenario', as there is no alternative case to the 'baseline'?

Section 2: Mention explicitly that the metrics are not time-invariant with respect to the time of the emission, and this leads to that the metric is defined as a function of t_e and t.

Rows 107 – 124: The notation (e.g. AM, R_{max}) is not explained.

Figure 2: Undefined expressions T_{i1}, T_{i2} and $d\Delta T/dt|_{rc}$

Equations (1) to (3): AM_x needs to be indexed with regard to i (as is done in eq. 4)

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2017-25, 2017.