Earth Syst. Dynam. Discuss., 6, C334–C338, 2015
www.earth-syst-dynam-discuss.net/6/C334/2015/

Earth System Dynamics
Discussions

Interactive comment on "Metrics for linking emissions of gases and aerosols to global precipitation changes" *by* K. P. Shine et al.

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Received and published: 5 June 2015

My review is mainly from the perspective of emission metrics. I think that this is a long-awaited paper in this field, and I have read the paper with great interest. This study illustrates the concept of an emission metric for precipitation changes, the Global Precipitation-change Potential (GPP), and explores its fundamental behaviors on short to long time scales, for five gases and aerosols characterized by vastly different atmospheric residence times and radiative properties, and with pulse and sustained emissions. The new metric is also tested under more realistic settings that actually apply the global emissions in year 2008. Their analyses reveal several new interesting and important issues that would arise when different emissions are compared in terms of precipitation changes, which they demonstrate are markedly different from more

conventional comparisons using temperature changes and radiative forcing. Those include but are not limited to the difficulty in using CO2 as a metric reference gas with short time horizons (due to the competing forces of the T- and RF-terms), strong contrasting impacts of black carbon on temperature and precipitation, and the leading role of methane in increasing the precipitation in the realistic 2008 case using sustained emissions. Throughout the paper, the GPP is well placed in comparison with more conventional counterparts such as the Global Warming Potential (GWP) and the Global Temperature change Potential (GTP). The theoretical derivation in Section 2 allows one to follow complicated behaviors of the GPP presented in the numerical section, which are in fact more complicated than I expected, considering the fact that the GPP is just about global-mean changes. A policy application of the GPP that I would conceive if I may is to use this index in parallel with other metrics in the climate stabilization context. Setting a time horizon for the GPP would be challenging because the climate stabilization does not explicitly target toward any level of precipitation changes (cf. (Johansson 2012; Tanaka et al. 2013)), but the GPP would be potentially useful to inform the underlying precipitation changes as a consequence of the temperature stabilization. This is a well-written piece of work with lots of thoughts, and I think it is an important contribution to the field of emission metrics and beyond, and will bring discussions to new heights. In conclusion, I support a publication of this paper in Earth System Dynamics. Below I provide several comments, which I hope are useful to refine the manuscript further, but all of them are admittedly minor.

1. It is somewhat pedantic, but I think that some introduction to emission metrics at the beginning of the paper would better inform a wide readership of what the paper is about. The current manuscript will not discuss emission metrics in general until Section 4.1. Most of the discussion in Section 4.1 can be moved to the introduction.

2. Along the similar line, the definition of the GPP can be made more explicit either in the abstract or somewhere upfront in the paper. As I read through the paper, I gradually see that the GPP is defined as a point-in-time metric (like the GTP) rather than an

integrated one (like the GWP), which is a crucial piece of information for the paper. More importantly, it would be helpful if the paper discusses why the GPP is formulated in this way. In other words, I wonder why the point-in-time formulation is adopted for the GPP even though there may be needs for a precipitation metric addressing a damage over a certain period of time, which would be captured better by a time-integrated precipitation metric.

3. (Shine 2009) tells an anecdote about how the GWP made the way to the Kyoto Protocol even if it had been initially meant only to illustrate difficulties inherent in the concept. While a more full account of what has actually happened is clearly needed in my view, one indication is that it is worthwhile to emphasize the purpose of a metric. In page 733, the manuscript states “these time horizons are chosen for illustrative purposes, rather than being indicative that they have special significance, except insofar as 100 years is used for the GWP within the Kyoto Protocol”, but I think that the paper can emphasize it more for example by stating something equivalent in the caption for Table 1.

4. In Section 4.1, where the background discussion is provided, I suggest the following (or something similar) to integrate a few more previous works in the discussion. “There have been attempts to derive metrics numerically from emissions pathways (Tanaka et al. 2009; Wigley 1998). Such metrics can be related to other analytical metrics under idealized settings (Cherubini et al. 2013).”

5. In Section 6, I found that the treatment of uncertainties in the GPP is limited. Although this study does address a few representative parts among others (i.e. intra-model variations and, more importantly, radiative partitioning) and the current approach suffices in my view, I would recommend some additional discussion to elaborate the nature of the uncertainties estimated in this study. The uncertainty ranges arising from the differences among models are known to be less comprehensive than those from the parameter ranges constrained by observations because the models are essentially best models based on best guesses for parameter values and do not usually bet for

less likely parameter combinations. This point has been shown in the metric context by (Reisinger et al. 2010). Furthermore, the carbon cycle uncertainty, which can be important given the behavior of AGPP, is not discussed.

6. Please elaborate how equation (5) is derived from equation (3).

7. A few errors spotted: page 732, Reisinger et al. 2013 (rather than 2012); page 734, line 16, “its emission are”;

References

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