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Comment

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

### **Anonymous Referee #3**

Received and published: 18 June 2015

This is a very useful and very interesting paper that uses the latest understanding of the relationship between the global energy budget and global precipitation to produce two global precipitation metrics that climate policy makers will find useful.

The manuscript is generally well presented and the figures and tables are quite useful. I did in places find the manuscript overly technical and I recommend some minor reordering and also have a few minor corrections.

1. The manuscript was let down but its abstract which I think did not do a very good job summarising the paper and was not particularly clear. In the first paragraph of abstract you say "Nevertheless, the GPP presents a useful measure of the global-mean role of emissions" but never really say why it is useful. The important sentence of regional effects seems out of place as impacts have not been talked about and you are sort of

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apologising for not measuring an impact when it wasn't clear that you were trying to - see also comment 2.

2. In the second paragraph you say "the GPP is further down the cause-effect chain from emissions to impacts than the GWP and GTP". I don't see this argued in the paper and would disagree -seeing that impact is so regional. I would place GPP at the same level as GTP. Generally the paper could do with being much more explicit that impact and risk relates to regional precip., but your metric is only global. I thinking justifying the introduction of GPP because it is more closely related to impact is on dangerous ground.

3. In the third paragraph of the abstract the sentence on BC splits two sentences discussing co2 as a reference gas - this did not read well. By the fourth paragraph of the abstract I have forgotten what the 5 species were. Generally the abstract could be much improved.

4. The metric discussion in section 4.1 would benefit from being much earlier on in the paper

5. The Appendix is referred to for the derivation of GPP but in fact the Appendix derives GTP, whose definition has already ben published and GPP is only obliquely mentioned in the Appendix. Maybe have both GTP and GPP equations or just the GPP ones?

6. Section 6 is overly long, especially when discussing cv. given the preliminary nature of the work is such detail needed?

7. Table 5 is mentioned in the text, P738, line 19 but does not exist

8. In tables, 1 2 and 3 especially and maybe in the text as well it was not clear if AGPP or GPP was meant. The tables seem to all be the absolute values but the AGPP acronym is not used consistently

9. It might be a good idea to show a 10 year value of AGPP in the tables with negative CO2 values, to clearly illustrate the change in sign issue?

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10. page 725, line 20. In my mind it is really important that rapid adjustment effects are accounted for - these affect the radiative heating of the troposphere. If you exclude them your RF response would be wrong. The text here makes the ERF approach appear like an inferior choice. Maybe I am being picky!

11. As the lead author has made a sustained contribution to metric research I suggest you change the Acronym of GPPs to KPS. So as not to embarrass Professor Shine it could stand for Kvalevåg based Precipitation metric for a Sustained emission?

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Interactive comment on Earth Syst. Dynam. Discuss., 6, 719, 2015.

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