Response to Reviewer Comments on Shine KP, Allan RP, Collins WJ, Fuglestvedt, JS 2015: Metrics for linking emissions of gases and aerosols to global precipitation changes Earth Syst. Dynam. Discuss., 6, 719-760 doi:10.5194/esdd-6-719-2015

Original reviewer comments are in normal font, our replies are in ***bold italics***. Intended changes to text are shown in **bold** font with quotes " ..."

Reviewer 1

The authors attempt to develop metrics for global precipitation changes in responses to various emissions, based on simple energy budget equations. The referee understands the usefulness of such metrics. The attempt to build up these metrics is highly valued. The major concern is that the simplification used in deriving these metrics bypasses the interactions of convective processes and the general circulation that remain the largest uncertainty in the model-based estimates of global climate precipitation changes. Using energy budget equations, these convection-associated processes are reduced to parameters as the efficiency of surface temperature and radiative forcing changes to precipitation ... I believe the authors understand the above simple arguments on the importance of convection variability on the global precipitation changes. It is however very difficult to rationalize such simplification from a convection/precipitation perspective.

While the reviewer recognises the usefulness and value of the metrics developed in our manuscript, unlike the two other reviewers, the reviewer has little else positive to say about the paper and does not provide constructive suggestions on how to improve the analysis.

The reviewer is, of course, correct that large and small scale dynamical processes are important in determining local rainfall rates, and changes in these rates; these are driven mostly by local convergence of moisture rather than a local balance between evaporation and precipitation. And these, of course, depend on the local circulation and changes in this circulation. This is well- and long-understood meteorology.

But we feel it is an extreme view to dismiss the global constraints on precipitation and precipitation change as irrelevant. There simply has to be a global constraint that evaporation balances precipitation and, similarly, there must be a global constraint between the atmospheric radiative divergence and the latent and sensible heat fluxes from the surface, and the changes in these variables. The power of this conceptual framework is illustrated in the work of Thorpe and Andrews (2014) and by our Equation 4 which shows that the relationship between global-mean precipitation change and global-mean temperature change across models contributing to CMIP-style intercomparisons can be largely explained using our simple energetic viewpoint. This is in spite of all the differences in convective parameterisations across these models that so concerns the reviewer. Indeed, two recent studies indicate that the energetic framework is also a valuable tool for understanding precipitation changes on more regional scales (Muller and O'Gorman, Nature Climate Change, 2011 10.1038/NCLIMATE1169 and Bony et al, 2013, Nature Geoscience 10.1038/NGEO1799).

In addition, the further comments by the reviewer concerning the inapplicability of the conceptual global-mean framework is undermined by the results of Andrews et al. and Kvalevag et al. which show that in spite of the inhomogeneity in some of the forcings, the same global-mean constraint applies. Yes, of course there may be non-linearities in the response to different forcings but the expectation here (supported by for example several geoengineering papers) is that the conceptual framework would still hold for global precipitation change.

From early in the abstract we acknowledge that information on global precipitation and precipitation change is limited, and it is impossible to argue with the viewpoint of the reviewer that impacts occur at the local scale. But this does not render the global view valueless. One could argue exactly the same point for local versus global temperature change. If the reviewer regards global temperature change as a valueless concept, especially given the large intermodel spread due (primarily) to uncertainties in cloud feedbacks, then so be it, but there are many who would disagree.

Finally, we consider that our work is the first to directly link emissions with precipitation response and we are disappointed that the reviewer was unable to recognise anything positive in this novel perspective.

We do not think there are many changes to our paper that would remotely satisfy this reviewer except to emphasise the power of the simple approach in explaining the results from the complex models (see Proposed Additional Comment 2 at the end of this response), despite large intermodel differences in representation of convection. We add, in the text following Equation (4)

"Hence Eq. (4) acts as a further validation of the utility of Eq. (3) for simulating global-mean precipitation change across climate models with varying parameterisations of, for example, convection, with climate sensitivities varying across the range from about 0.4 to 1.3 K $(W\ m^{-2})^{-1}$"

Other than that we focus our revisions on the more constructive comments of the other reviewers.

Reviewer 2

[...] 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.

We thank the reviewer for the many positive and supportive comments.

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.

Thank you for this comment, which we do not regard as pedantic – the location of this material was an active topic of discussion amongst the authors which resulted in its placement after it had started off in the introduction. The fact that Rev2 and Rev 3 make the same point strongly argues for moving the discussion back to the introduction, which we have done in the revised version.

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.

We agree that we should be clearer that the GPP is presented as a "point-in-time" (we prefer the nomenclature "endpoint") metric within the abstract and also in the text at the end of section 2. It is a very good question as to whether an end-point metric is better than a time-integrated one, and we feel there are arguments on both sides. We certainly agree that discussion of a time-integrated perspective is valuable. In the revision we highlight (in the abstract, text and conclusions) a very important result (stressed by Peters et al. 2011 and also by others), that a sustained metrics (such as the GTP_S) can equally be regarded as mathematically equivalent to the time-integrated pulse metric (such as the GTPp), and so allows alternative interpretation. This interpretation carries over to the GPP. We have rewritten the GTP_S expressions in the Appendix into the more compact form used by Peters et al., as in this form it is much more apparent that the GTP_S is the time-integrated GTPp.

The new text at the end of Section 2 reads

"Note we have chosen to present the $AGPP_P$ and $AGPP_S$ as end-point metrics – i.e. as the effect at the time horizon H of an emission at (or starting at) time zero. For some purposes, a time-integrated metric might give a useful perspective. Following Peters et al. (2011 – see in particular its Supplementary Information) we note that the time-integrated pulse metrics are mathematically equivalent to the end-point metrics for sustained emissions. Hence, the $AGPP_S$ and GPP_S can equally be interpreted as time-integrated forms of the $AGPP_P$ and GPP_P."

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.

We add a statement in all Tables to state that the chosen time horizons are for illustrative purposes.

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).”

We agree with this comment and incorporate this as part of the discussion (which will be moved to the Introduction as noted in Comment 1 above).

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. intramodel 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.

Reviewer 2 and 3 have somewhat divergent views on Section 6, with Reviewer 2 feeling it is limited and Reviewer 3 feeling it is overlong. Given that this reviewer concludes that it “suffices” we will keep it much as it is, but seek to trim words to help with Reviewer 3’s comments. We did indeed briefly mention the carbon cycle uncertainty at line 5 page 736, but we will slightly expand this discussion and refer to Joos et al. (ACP 2003). We will also note that using model uncertainty range may not properly straddle the true uncertainty range.

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

We will add at the beginning of the sentence that “**Since more generally $\Delta T_{eq}=\lambda RF_{eq}$, Eq. (3) can be written ...*****”***

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

Thank you – these are corrected in the revised manuscript.

Reviewer 3

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.

We thank the reviewer for the many positive and supportive comments.

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

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.

These are all important comments and we are obviously concerned if the abstract does a poor job. We feel that the reviewer's perspective that the GPP is at the same level as the GTP is insightful, and perhaps the conventional cause-effect chain (see e.g. Figure 8.27 of Myhre et al. (2013)) should be modified so that the "climate change" box is split into two, with "global climate change" first and then "regional climate change" following it (and before "impacts") – that discussion is for another day/paper, but we will revise the abstract accordingly to give GTP and GPP equivalent billing and down play the greater relation to impact. Although the "apology" (see point 1) is less needed in this case, we still feel it needs spelling out for less-expert readers, as precipitation changes are so different in nature to temperature change. We also amend the fifth paragraph so the abstract now reads:

"Recent advances in understanding have made it possible to relate global precipitation changes directly to emissions of particular gases and aerosols that influence climate. Using these advances, new indices are developed here called the Global Precipitation-change Potential for pulse (GPP_P) and sustained (GPP_S) emissions, which measure the precipitation change per unit mass of emissions.

The GPP can be used as a metric to compare the effects of emissions. This is akin to the global warming potential (GWP) and the global temperature-change potential (GTP) which are used to place emissions on a common scale. Hence the GPP provides an additional perspective of the relative or absolute effects of emissions. It is however recognised that precipitation changes are predicted to be highly variable in size and sign between different regions and this limits the usefulness of a purely global metric.

The GPP_P and GPP_S formulation consists of two terms, one dependent on the surface temperature change and the other dependent on the atmospheric component of the radiative forcing. For some forcing agents, and notably for CO_2, these two terms oppose each other – as the forcing and temperature perturbations have different timescales,

even the sign of the absolute GPP_P and GPP_S varies with time, and the opposing terms can make values sensitive to uncertainties in input parameters. This makes the choice of CO_2 as a reference gas problematic, especially for the GPP_S at time horizons less than about 60 years. In addition, few studies have presented results for the surface/atmosphere partitioning of different forcings, leading to more uncertainty in quantifying the GPP than the GWP or GTP.

Values of the GPP_P and GPP_S for five long- and short-lived forcing agents (CO_2, CH_4, N_2O, sulphate and black carbon (BC)) are presented, using illustrative values of required parameters. The resulting precipitation changes are given as the change at a specific time horizon (and hence they are end-point metrics) but it is noted that the GPP_S can also be interpreted as the time-integrated effect of a pulse emission. Using CO_2 as references gas, the GPP_P and GPP_S for the non-CO_2 species are larger than the corresponding GTP values. For BC emissions, the atmospheric forcing is sufficiently strong that the GPP_S is opposite in sign to the GTP_S. The sensitivity of these values to a number of input parameters is explored.

The GPP can also be used to evaluate the contribution of different emissions to precipitation change during or after a period of emissions. As an illustration, the precipitation changes resulting from emissions in 2008 (using the GPP_P) and emissions sustained at 2008 levels (using the GPP_S) are presented. These indicate that for periods of 20 years (after the 2008 emissions) and 50 years (for sustained emissions at 2008 levels) methane is the dominant driver of positive precipitation changes due to those emissions. For sustained emissions, the sum of the effect of the 5 species included here does not become positive until after 50 years, by which time the global surface temperature increase exceeds 1 K. "

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

See Reviewer 2, comment 1 – we will move the discussion (back!) to the introduction.

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?

We have improved the link between the Appendix and the equations in the main text describing the GPP, so it is clearer how the GPP is derived. We have also acknowledged where expressions have been published before.

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

Reviewer 2 and 3 have somewhat divergent views on Section 6, with Reviewer 2 feeling it is limited and Reviewer 3 feeling it is overlong. Given the view of Reviewer 2, and the fact that uninterested readers can easily skip this section, we retain it, but have sought to trim the number of words by about 10%, and remove any extraneous information.

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

Apologies and thank you – it should have been Table 3 (Table 4 now)

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

This is the format that was used in the original GTP paper and we thought it clear that only CO_2 was labelled as "absolute" – however, we are concerned that this wasn't clear to the reviewer and so we have decided to present all the absolute metrics together in Table 1 and remove them from what were Tables 1, 2 and 3 (now Tables 2, 3, 4).

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?

While we like this suggestion, with the standard configuration we use, the $AGPP_P$ is positive at 10 years and hence it would not illustrate the sign issue. We highlight the sign issue in the text and in several figures (1, 3, 7, 8 and 9) and we add it to the issues raised in the abstract in our re-working of that, in line with this Reviewer's comments 1-3.

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!

We do not think this is being picky, although we are not entirely sure the comment refers to page 725, line 20 or which part of our text made ERF look inferior to RF. We have added additional text in Section 2 to make clear that a fully consistent approach would use the ERF, but we are not currently able to do this.

"Note that a fully consistent approach would adopt effective radiative forcings (ERF – see Myhre et al. (2013)) rather than RF, and values of *f* derived using ERFs. However, assessed values of ERFs are not available for many species and so, in common with Myhre et al., (2013), the metric values calculated here use RFs, but including a number of indirect chemical effects and some cloud effects, as noted in Section 3. The values of *f* are based on one method of deriving ERFs and a possible reason for differences between *f* values in Andrews et al. (2010) and Kvalevåg et al. (2013) is that the fast tropospheric responses that distinguish RF from ERF differ between the models used in their study."

The difference between RF and ERF could be a further reason why individual models depart from Equation (4) (as their fast response varies from model to model) and so we also note this. However, the values presented for CO_2 (which seems the specific concern of the reviewer) will not change if we follow Myhre et al. (2013) who state that for WMGHG "the ERF best estimate is the same as the RF" with a slightly larger uncertainty. We have noted this in the text preceding Equation 4.

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?

Thank you for this nice comment.

Proposed Additional Changes

In view of further discussions with colleagues since we submitted the paper, and a suggestion by the Editor, we propose to make three further changes

1. The present manuscript does not do a sufficient job of discussing the sensible heat (SH) flux changes and implies that these can be satisfactorily incorporated in the $k\Delta T$ term. While the $k\Delta T$ approach captures the slow response of SH (at least, in the model-mean sense, although there is not even a consensus as to the sign of the sensible heat flux change for even "simple" perturbations such as doubling CO_2 (Previdi 2010)), our text ignores the fast response of SH – indeed the text at lines 28-29 on page 724 confuses the two effects. We add an additional paragraph specifically on the fast and slow responses of SH

"ΔSH in Eq. (2) is less well constrained. It also has two components, one due to the fast response to RF, which is independent of surface temperature change, and one due to surface temperature change. The fast response has been shown to be small for greenhouse gas forcings; Andrews et al. (2010) and Kvalevåg et al 2103 show it to be typically less than 10% of ΔLH for a doubling of CO_2, although the size and sign varies can vary amongst models (Andrews et al. (2009)). However, it can be much larger for other forcings (of order 50% of ΔLH in the case of black carbon (Andrews et al. (2010) and Kvalevåg et al 2013)). As noted by Takahashi (2009) and O'Gorman et al. (2012) an improved conceptual model could distinguish between ΔR_d for the whole atmosphere and ΔR_d for the atmosphere above the surface boundary layer, as changes in ΔR_d within the boundary layer seem more effective at changing SH (e.g. Ming et al. (2010)) and hence less effective at changing LH. Here, following Thorpe and Andrews (2014), we assume the fast component ΔSH to be small and neglect it, but more work in this area is clearly needed."

2. We insufficiently stress that the simple conceptual model encapsulated in equations (2) and (3) does a good job of reproducing results from sophisticated climate models, which is essential for our work. The brief discussion of Thorpe and Andrews (lines 5-6, page 725) needs to be expanded to emphasize this point

"Despite its apparent simplicity, Eq. (3) has been shown by Thorpe and Andrews (2014) to simulate reasonably well future projections of precipitation change from a range of atmosphere-ocean general circulation models, albeit with a tendency to underestimate the multi-model mean. Uncertainty in the value of f for all forcing agents (and possible inter-model variations in f – see section 6) inhibit a full assessment."

and we further will emphasize that equation (4) also acts in a sense as a validation of the simple model by adding an extra sentence in the text following the equation to say

"Hence Eq. (4) acts as a further validation of the utility of Eq. (3) for simulating global-mean precipitation change across climate models with varying parameterisations of, for example, convection, with climate sensitivities varying across the range from about 0.4 to 1.3 K (W $m^{-2})^{-1}$"

3. We should note that Shindell et al. (ACP, 2012, 10.5194/acp-12-6969-2012) introduce a concept of a "regional precipitation potential" and this needs discussing in our introduction and conclusions – their concept is precipitation change per unit forcing (rather than per unit emission), and so differs conceptually to ours, and is more concerned with linking geographical patterns of forcing with geographical patterns of precipitation change. We had been unaware of this paper at the time of submission, until one of the authors of the paper drew (but not Drew) it to our attention. The new text reads in the introduction

"In more idealised experiments with one climate model, Shindell et al. (2012) have demonstrated a link between radiative forcing (due to a variety of forcing mechanisms) in specific latitude bands to precipitation change in a number of selected regions; their precipitation change per unit radiative forcing was called a "Regional Precipitation Potential", which is distinct from the framework here, where the precipitation change is directly related to emissions".

In addition, some minor proofing corrections made for ESDD are now incorporated in the word version.