Earth Syst. Dynam. Discuss., 3, C506–C509, 2012
www.earth-syst-dynam-discuss.net/3/C506/2012/

Earth System Dynamics Discussions

Interactive comment on "A synthesis of climate-based emission metrics with applications" *by* B. Aamaas et al.

D.J.A Johansson (Referee)

daniel.johansson@chalmers.se

Received and published: 11 October 2012

General Comment: This is a valuable paper in the sense that it summarizes clearly the approach of using linear impulse response functions (IRFs) to estimate the GWP, GTP and iGTP/IGTP emission metrics. The title of the paper is somewhat misleading since it is not directly a synthesis; it is more of a background paper on how emission metrics are calculated. Given this the novel issues are limited. However, it is useful piece since it presents how emission metrics based on IRFs are calculated in a systematic and clear way. This has been lacking in the scientific literature. The paper would benefit from a discussion about other approaches to compute metrics such a using reduced complexity carbon cycle and energy balance models instead IRFs. Such approaches are used in a range of metric studies (e.g. Reisinger et al, 2010, Tanaka et al, 2009,

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

Azar & Johansson, 2012). A clear justification why the paper is limited to an IRF based approach would be valuable.

Specific comments: Page 873, line 5-7: The authors write “A limitation of using RF directly is that it does not capture the transient response in the atmospheric concentration when medium to long-lived gases are studied.” This is a little bit unclear. I would say that RF never captures any transients, whether the forcer/gas is short-lived or long-lived. Dynamics is not accounted for in the RF concept as such. With “transient response” I think the authors refer to atmospheric perturbation time of the forcers but I am unsure if they also want to include something more in this.

Page 876, line 18-19: The authors write “The choice of reference gas is diïňČcult, and the long term behavior of CO2 is one of the main reasons for needing a value-based TH in normalized emission metrics (IPCC, 1990; Lashof and Ahuja, 1990).”. I agree on that the choice of reference gas is difficult, but I find the argument that “the long term behavior of CO2 is one of the main reasons for needing a value-based TH in normalized emission metrics“ is somewhat peculiar. Of course, TH has a large importance on the absolute metric value for CO2 and consequently for the normalized metric when CO2 is the reference gas, but arguing that CO2’s perturbation lifetime is “one of the main reasons for needing a value-based TH” seems to be reverse logic. If we value future impacts differently than impacts today a valued based TH would be needed independent on the life time of CO2. Reducing the issue of TH to a mere practical issue on how to deal with the long term response of CO2 is to simplify the discussion too much.

Page 881, line 16-18. The authors write “The short integrations in CMIP3 make it diïňČcult to estimate the longer time constant, and, hence, the climate sensitivity derived from the IRFs diïňĂers from the climate sensitivity of the climate model (Olivie et al., 2012)”. I have not read the paper by Olivie et al. (2012) (seems interest though and I plan to read in the near term future), but one should at least be able to estimate the “effective climate sensitivity “ from the CMIP3 experiments. This value may how-

ESDD

3, C506–C509, 2012

ever be somewhat different from the “equilibrium climate sensitivity”. I presume that the interesting comparison is between the “effective climate sensitivity “and the parameters derived for the IRFs and not between the “equilibrium climate sensitivity” and the parameters derived for the IRFs.

Page 881 line 16 – page 882 line 11. I think the discussion here would benefit from a more general discussion about the relationship between the response time of the climate and climate sensitivity. As is well known from rather old literature there is a strong relationship between climate response time and climate sensitivity , see for example Hansen et al, 1984 and Harvey (198X). This aspect is also analyzed in the context of iGTP/IGTP in Azar & Johansson (2012). As the discussion is in the paper now it is rather hard to follow and may cause some confusion.

Page 884 line 16-27: I find this discussion somewhat confusing and I cannot really see the point with it. Can the authors suggest any kind of practical impact assessment where it would be relevant to base the radiative efficiency and impulse response function on pre-industrial conditions?

Page 886, line 21: The authors write “..this context are emissions linked to ozone formation or destruction,..” Please write out that you refer to TROPOSPHERIC ozone (this is at least how I read it).

Page 888, line 17. I think “altitude” should be “latitude” here.

Page 890 equation 18. I think “,” before “{“ is misplaced.

Page 895 line 9. Although I do not want to push my own work I think a reference to Azar & Johansson (2012) is relevant here.

Page 898 line 15-18. I think the piece of text is somewhat misleading, Manne and Richels did not investigate GWP, they did suggest the GCP (although not using that acronym).

Page 902 line 20: A reference to Azar & Johansson (2012) could be relevant here as

ESDD

3, C506–C509, 2012

well.

Page 906 line 1-3. The authors write “In general, the climate impact is governed by species with strong, but short-lived impact and weak, but long-lived impacts”. What do you want to suggest with this statement? SF6 for example is both strong and long-lived?

References (in addition to those which are in the paper)

Hansen J., Russell G., Lacis A., Fung I., Rind D., and Stone P.: Climate Response Times: Dependence on Climate Sensitivity and Ocean Mixing, Science 229 (4716): 857-859, 1985.

Johansson D.J.A., Azar C., 2012, On the relationship between metrics to compare greenhouse gases – the case of IGTP, GWP and SGTP, Earth System Dynamics Discussions (ESDD)(3: 113–141. (Accepted for publication in ESD)

Tanaka, K., O’Neill, B.C., Rokityanskiy, D., Obersteiner, M., Tol, R., 2009. Evaluating Global Warming Potentials with historical temperature. Climatic Change 96, 443-466.

I hope you do find my comments useful!

Daniel Johansson

Interactive comment on Earth Syst. Dynam. Discuss., 3, 871, 2012.