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Interactive comment on "Evaluating and improving the treatment of gases in radiation schemes: the Correlated K-Distribution Model Intercomparison Project (CKDMIP)" *by* Robin J. Hogan and Marco Matricardi

Anonymous Referee #1

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General comments:

The paper describes a valuable intercomparison of modern radiation methods used in NWP and ESM codes with a focus on the underlying formulation of the correlated k-distributions (CKDs). The paper describes the rationale for the MIP protocols, describes how interested groups may participate and contribute their results, and demonstrates the application of these protocols to a particularly widely-used CKD-based code, the Rapid Radiative Transfer Method for GCMs (RRTMG). The topic and nature of this article are both good fits for GMD.

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Discussion paper

I would recommend acceptance subject to minor revisions – my principal concern is that the protocol is strongly oriented around WMGHGs but should have a much simpler way of attributing errors to the treatment of water vapor, which after all is by far the dominant radiatively active species in both shortwave and longwave.

Major Comments:

1. The paper would benefit from citation of recent work in the introduction that demonstrates that there is still substantial room to improve the accuracy of radiative transfer (RT) codes used in climate applications. While not all the codes cited in these works are based on CKD, CKDMIP would not be underway if the accuracy of RT parameterizations relative to line-by-line (LBL) codes was a settled problem. Recent papers that could be cited include Soden et al (2018) that showed that the range of instantaneous RF from doubling CO_2 exhibits nearly the same range as it did in prior evaluations including RTMIP (Collins et al, 2006) and going as far back as Cess et al (1993). Other relevant citations include DeAngelis et al (2015) and Fildier and Collins (2015), both of whom showed that inaccuracies in the parameterizations of near-IR absorption by H_2O introduced significant spread in the response of the hydrological cycles simulated by the parent climate models to global warming.

2. Similarly, the paper would benefit from summarizing the codes used by current Earth System Models as configured for the 6th Coupled Model Intercomparison Project (CMIP6). These codes are documented in detail at https://es-doc.org/. Shockingly, there are CMIP6 codes using 30+ year-old shortwave parameterizations and longwave parameterizations based on elaborations of cooling-to-space formulations. Even for codes using CKD in their radiation suite, there are diverse formulations of CKD and this technique is sometimes used for one band (typically longwave) rather than both shortwave and longwave bands.

3. The design of the CKDMIP, particularly the first and second evaluation data sets

(table 1 and 2) containing randomly selected realistic thermodynamics profiles, is going to make it much harder to search for systematic errors associated with the treatment of water vapor than is necessary or feasible. Since water vapor is by far the dominant radiatively active species in both the shortwave and the longwave, and since the literature cited in the first major comment above shows that there is still significant spread in the accuracy of the parameterization of near-IR H2O absorption across the CMIP ensembles, this is my principal concern regarding the design of CKDMIP. It's really important to be able to look at the change in the k distributions and resulting fluxes and heating rates when water vapor **alone** is perturbed. There is a simple fix for this, fortunately, if CKDMIP were to also ask for the exact same set of data from each contributing group for the *idealized* profiles as for the *Evaluation-1* and, ultimately, *Evaluation-2* datasets.

4. Otherwise attribution of errors associated with overlap of WMGHGs and H_2O – e.g., CO_2 and H_2O, CH_4 and H_2O, etc. etc – is going to be challenging to put it mildly.

5. The article is curiously silent about the extensive literature on how best to construct CKDs. It should cite existing exhaustive literature on methods, with extensive contributions from the combustion engineering and astronomical communities. A brief search of *JQSRT* should suffice to produce some recent works to cite.

6. Aside from the somewhat empirical objectives of CKDMIP, the authors should consider posing the moderately rhetorical question why isn't there a formal theory for how to, e.g., satisfy cost functions (error limits) with a minimum number of points, that works for the widest possible range of plausible conditions, e.g., mass paths ranging from zero to infinite? After all, CKD is simply a discretization of the Laplace transform of the non-grey Beer-Lambert law – it should be possible to formulate a proper mathematical theory for how to implement this discretization.

7. There is a fair amount of meta-data that should be recorded by each contributing group and it's not clear from the experimental protocols in section 4 that its collection is mandated. Are the provenance of the line data recorded? Assumptions re line width? Variations in continua? Resolution of underlying LBL calculation used to generate k(g)s? For given interval in g, how is k chosen?

Minor Comments:

1. Line 406: Why not require mapping to k intervals at the native resolution of the LBL results used by each group? As noted in the article, this would be a vector of approximately 7 million integers for the longwave and 3 million integers for the shortwave, i.e., a trivial but extremely useful volume of data.
2. Line 19: CKD is implemented via a Laplace transform of the Beer-Lambert law followed by reformulation in terms of a cumulative integral – this description in the current version make CKD sound little better than exponential sum fitting.
3. Line 46: Lack of formal theory issue raised in major remarks above.

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