Comments to the manuscript by Lopez-Puertas et al "An improved and extended parameterization of …"

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OVERVIEW

The paper presents the revised Fomichev et al. (1998) matrix parameterization of the CO2 15 μm cooling rates of the Earth's middle and upper terrestrial atmosphere for both local thermodynamic equilibrium (LTE) and non-LTE layers. It is essentially based on the same approach as the Fomichev et al. (1998) parameterization. The main improvement is an extended range of CO2 abundances: whereas Fomichev et al. (1998) routine covered the range of CO2 concentrations with tropospheric values from 150 to 720 ppm the parameterization presented in the paper goes up to 3000 ppm of tropospheric CO2. Another minor improvement is the finer altitude grid of revised parameterization.

GENERAL COMMENTS

This is a large manuscript with very large numbers of plots both in main body and appendixes. Although it is well written and structured, many tests of a new routine repeat one another and, therefore, look excessive.

The work takes us back to the late 70s - early 80s of the last century, when new for that time techniques of the approximate 15-micron CO2 cooling calculations (both for LTE and non-LTE conditions) were developed, see (Kutepov, 1978) and (Akmaev and Shved, 1982). The revised version of the Fomichev et al, 1998 (hereafter F98) routine described in the manuscript does not represent any innovation and does not suggest any new option for the GCM users and developers.

This statement may be explained as follows. Any physical parameterization for GCM must be able to react as realistic as possible at steady changing local physical state of the atmosphere in the modeling process. This is particularly true for the infra-red radiation and its effects (local cooling or heating), which are critically important for adequate modeling of energy balance. It is well known that instantaneous p-T distributions in modern GCMs of middle and upper atmosphere exhibit very strong variability, caused by the superposition of tidal and gravity waves of different amplitude and vertical scales. Same is true for the atmospheric p-T distributions measured in both ground and space experiments. Therefore, the parameterization of the 15-micron CO2 cooling, which is a main radiative cooling mechanism of these layers, must properly react to this variability.

However, the matrix parameterizations of the 15-micron cooling are unable to provide adequate reaction to strongly disturbed T distributions. This was well known already 30 years ago for those (I was among them) who worked on developing the first version of the F98 parameterization. Demonstration of large parameterization errors for wavy T profiles was not included in the paper by Fomichev et al, 1993, however, it contained at least the warning addressed to its users ***"It is recommended for the calculation of the radiative cooling for smooth temperature profiles, namely for profiles undisturbed by micro- and meso-scale motions."*** When the updated 1993 routine was released by Fomichev et al, 1998 nothing changed, the revised routine was still unable to treat wavy distributions, see (Kutepov and Feofilov, 2023, hereafter KF23). Again, as in the 1993 paper, the accuracy of F98 was demonstrated exceptionally for smooth T profiles. However, the authors dropped from the paper text the warning for its users cited above. Since that time the F98 routine has been widely used in GCMs of middle and upper atmosphere for any T distributions.

It is also worth noting here that 15-micron CO2 radiative cooling is strongly non-linear in respect to the temperature variations. Therefore, the zonal mean cooling, which is enthusiastically discussed in this manuscript, is not equal to that calculated for the zonal mean temperature. The authors, however, pay no attention to this fact!

Now back to the current manuscript. The authors invested large efforts to update the F98 routine. They declare that the revised routine (hereafter L-P.23) allows calculations of 15-mucron cooling with higher accuracy for current CO2 vmr. It calculates with reasonable accuracy also the cooling for much higher CO2 concentrations. Nevertheless, again the accuracy of L-P.23 is demonstrated only for very smoothed "standard" T distributions.

Meanwhile, the authors are aware about large errors F98 routine has for disturbed T profiles. D. Marsh was the co-investigator of a recent NASA grant (Kutepov, 2021), where I was the PI. He and his team received funding for testing the KF23 algorithm for calculating 15-micron non-LTE cooling, comparisons of this routine with F98 parameterization, and for installing KF23 routine in the WACCM model. This study showed that F98 causes very large cooling errors (up to 25 K/day) on wavy profiles. These errors are discussed in KF23.

Meanwhile, the authors of the manuscript are quite honest when, describing the main motivation of their study, they write: ***"In our case we have the option of developing a completely new parameterization, to adapt other CO2 parameterizations (as those cited above), or to extent and improve the parameterization of Fomichev et al. (1998). Attending mainly to practical reasons of promptness, we opted for the later."*** The keyword here is "***promptness".***

This "***promptness"*** looks somewhat strange after 25 years of no interest of the authors to the problem of fast and accurate calculation of radiative cooling for the Earth's GCMs. Knowing about the drawbacks of F98 routine and other matrix parameterizations we spent these years developing accurate non-LTE radiative transfer techniques, which are free of these drawbacks and are fast enough to be applied in GCMs. The results of this long-term study are summarized in KF23.

Finally, what kind of new product these authors developed with the main motivation "to be prompt"?

Again, I do not need my own judgement. It is enough to cite what the manuscript authors write when they describe large errors, much lagers than 0.5-1.0 K/day reported for standard profiles, which they observed for all profiles at latitudes northernmost of 50 N for a single non-standard situation with a pronounced elevated stratopause event:

"***Both parameterizations underestimate the cooling in that atmospheric region. The new parameterization has, however, a better performance above about 80 km, but in the strat-warm/elevated stratopause region (80–100 km) it still underestimates the cooling by 3–7 K Day−1 (∼10%)".***

This looks like a confession that in a non-standard situation the new l-P.23 routine works no better than F98.

And further: ***"It seems clear that part of this underestimation is caused by the fact that such atypical temperature profiles (see Sec. 3.1) were not considered in the parameterization. However, its inclusion would not solve the problem as in the calculations of the coefficients a trade-off of the weighting of the different p -T reference atmospheres have to be chosen (see Secs. 5.1 and 5.2). Thus, it might ameliorate the inaccuracy for these elevated-stratopause events but would worsen the accuracy for other general situations. This manifests the difficulty/limitation of this method to provide accurate non-LTE cooling rates for all temperature structures (gradients) that we might find in the real atmosphere."***

I absolutely agree with this statement: this approach for parametrizing the 15-micron non-LTE cooling in the middle and upper atmospheric layers, which was applied in previous routine versions in 1993, in F98, and repeated in L-P.23 **is a deadened approach**.

In KF23 we discuss in detail its drawbacks. Briefly: it is impossible to adequately estimate the non-LTE cooling in the very variable atmosphere by dividing it in several altitude regions, where different techniques or expressions for cooling calculation (although linked in various ways) are used. Only exact algorithms, which rigorously describe the radiative energy exchange between various altitude layers and the non-LTE radiative field coupling with atmospheric heat reservoir may satisfy the current cooling accuracy requirements.

It is my opinion that this paper in its current shape must not be published. It does not provide any significant improvement compared to previous work(s).

To be published the manuscript requires significant major revision:

(1) it must demonstrate how revised routine works for T profiles disturbed by the strong waves. If the routine fails on the wavy profiles, but the authors still recommend it for further usage in GCMs, then (2) they need to justify that these errors have negligible or no effects on the GCM model results.

SPECIFIC COMMENTS

1. ***Why exact methods of the non-LTE CO2 cooling calculation cannot be applied in GCM?***

The manuscript authors write: ***The computation of the cooling under those non-LTE conditions requires the solution of the radiative transfer equation (RTE) which is a non-local problem and requires a large amount of CPU time. Therefore, the solving the RTE in general circulation models (GCMs) or climate models that extend in height above the stratopause is***

impractical and efficient parameterizations of the CO2 infrared cooling have been developed and implemented 30 in such models.

I disagree with this statement. It does not matter whether the LTE approach is applied or the non-LTE problem is considered, in both cases the calculation of cooling requires the solution of RTE, and is, therefore, the non-local problem.

Moreover, the computing costs of exact RTE solution in modern algorithms are not the main problem, which makes "***the solving the RTE in general circulation models GCMs impractical".*** The authors mean here not just RTE but the entire non-LTE problem. Inversion of large matrices to get the populations in the developed in 1950s CM (Curtis, 1956) matrix algorithm, which the members of this team utilize since 1980s, is most computational costly part of the non-LTE problem solution. The authors either are not aware of or ignore dramatic progress in the developing the non-LTE techniques, see, for instance, (Hubeny and Mihalas, 2015) and (Frish, 2021). Large matrix inversion costs make CM technique usage impractical in GCMs. We discussed this in the papers by Kutepov et al, 1998, Gusev and Kutepov, 2003, and in more details in KF23.

2. **How the 15-micron cooling maximum in the lower thermosphere is formed?**

In the manuscript**: *Above the mesopause, the cooling rate rapidly increases following the enhancement of the kinetic temperature. Above about 130 km, the cooling rates decline because of the depletion of the CO2 vmr (see Fig. 7).***

Are the authors sure about this? I recall that in early publications of the 1980s about the 15-micron cooling it was demonstrated that cooling declines at higher altitudes even for constant CO2 vmr. Main problem at these altitudes is the rapid decrease of atmospheric pressure and the CO(v2) collisional quenching rate, which disconnects the 15-micron radiation from the atmospheric heat reservoir. If the authors do not agree with my comment, could they demonstrate that cooling stops decaying above 130 km when CO2 vmr is constant?

3. **Why the CO2-O quenching rate coefficient ~ 6.0e-12 cm3 s −1 was used as the basic one for revision of the F98 parameterization?**

The authors write that they used for updating the F98 parameterization the quenching rate coefficients which are very similar to those used for its development "***… except the k CO2−O rate (process 1c in Table 1) that has been considered here with its upper limit. That is, about a factor of two larger than in the parameterization of Fomichev et al. (1998). This rate coefficient is not well known with uncertainties of the order of a factor of two (see, e.g., García-Comas et al., 2008). While laboratory measurements are in the range of 1.5 to 2·10−12 cm3 s −1 the values derived from atmospheric observations are close to 6·10−12 cm3 s −1. …."***

First, measured in laboratory and retrieved from the atmospheric observation values of k differ not by a factor 2, but by 3-4. Additionally, if one accounts for the k retrievals by Feofilov et al, 2012, which involved the ground-based lidar temperature measurements, then tis factor will be 3-6.

Table 1 shows that the authors selected k ~ 6.0e-12 cm3 s −1 for updating the parameterization. Although k is supposed to be an input parameter in both F98 and L-P.23 routines, however, the previous one was optimized in the transition region from LTE to non-LTE for k ~ 3.0e-12 cm3 s −1, whereas new one is optimized for 6.0e-12 cm3 s −1. The authors tell in manuscript that this causes additional differences between F98 and L-P.23 in the transition area even when both apply the same rate coefficient, and then explain the reason why they selected higher rate for optimizing L-P.23 as ***"… we have optimized it for the high value (see Table 1), as this value has been used in the most recent non-LTE retrievals of temperature from SABER and MIPAS measurements".***

It is not enough, however, to say higher k was selected because this rate provides more reasonable T retrievals from space observation. As the authors know to validate these T measurements current GCMs apply twice lower rate 3.0e-12 cm3 s −1. If the authors recommend, which follows from the text, to use L-P.23 with the most high k, then can they demonstrate how this affects the GCM (for instance the WACCM model) runs compared to those with twice lower k? Does this provide better fitting of measured temperatures?

4. Testing the parameterization for measured temperatures

The Fig.21 of manuscript shows an example of the MIPAS nighttime temperature profiles (15 February 2009) used for verifying the parameterization accuracy. The authors note large variability of the measured temperature profiles. These individual profiles are the good inputs for the revised parameterization to show how it works for strongly disturbed T profiles.

The authors have obviously performed these tests, but they do not show these results. Instead, they write "***The results are presented in Fig. 22 for the zonal mean of the differences for two days of solstice and two days of equinox conditions and in Fig. 23 as the global mean difference for all latitudes for each of the four individual days***." Why only these mean values are shown? Obviously, the averaging smashes the errors obtained for individual profiles, for which we observed in our study of F98 parameterization presented in KF23 errors up to 25 K/day. Meanwhile these large errors in our study have generally concentrated in the altitude region around 90 km, exactly where the RMSs in Fig.25 of manuscript are maximized reaching 8-9 K/day. In our paper we explain why F98 works badly in this transition region. L-P.23 has the same problems and is nothing better.

5. The accuracy of L-P.23 for smooth T profiles

I wrote above that L-B.23 will not be any better than the F98 for disturbed T profiles. But how about the smoothed standard T profiles? The accuracy of cooling calculations with L-P.23 less than 0.5 K/Day for preindustrial CO2 for smooth profiles is also questionable.

(1) The non-LTE model, which is used in this study for the reference calculations to optimize L-P.23 and then to check its accuracy, includes only 18 15-micron bands. From my point of view this model itself is not accurate. In the routine we suggest in KF23, which utilizes the exact solution of the non-LTE problem, we use the same bands to calculate nighttime cooling for 400 ppm of CO2 with an error not higher than 1 K/day for any T profile, including disturbed by strong waves. For smooth profiles this error reduces to ~ 0.2

K/Day. These errors were estimated by comparing KF23 routine with our reference model, which comprises 60 vibration levels of 5 CO2 isotopic species and hundreds of bands. So, the exact algorithm compared to the exact algorithm showed 0.2 K/day error when a reduced set of bands was used. I doubt that the error of 0.5 K/day the authors report for the L-P.23 routine for smooth profiles after comparing it with a very simplified "reference model" is true. It must be higher.

(2) The authors write that contribution of the heating due to the absorption of solar radiation in the near infrared CO2 bands at day time is negligible compared to the 15-micron cooling. However, 2-3 K/day (for current CO2) do not look negligible for the routine, which accuracy is declared to be about ~0.5 K/day. We tested in detail the (Ogibalov and Fomichev, 2003) parameterization of this heating, found this warming increasing for some wavy T profiles, and made sure this parameterization cannot guarantee the error of the KF23 routine at daytime to be lower than 1 K/day. As a result, we extended our KF23 daytime model up to 26 CO2 vibrational levels and 56 bands to satisfy this requirement.

(3) The authors say nothing about how they account for the cooling effect of the micro-scale sub-grid T disturbances. Kutepov et al., 2007 and Kutepov et al., 2013 showed that these temperature fluctuations cause near the mesopause an additional cooling up to 3 K day-1. I draw the authors' attention to the results shown by Kutepov et al, 2013 in Fig.1.5. It demonstrates one of the runs of the Leibniz Institute Middle Atmosphere (LIMA) model with the 15-micron cooling modified to account for the sub-grid T disturbances. It is shown that very minor variation of cooling (not higher than 2-3 K/day) lead to significant changes of the monthly and zonal mean temperatures for July 2005.

I am sure that errors of L-P.23 routine are much higher than 2-3 K/day and can be hardly reduced due to the deficiencies of the methodology applied. These errors will obviously have a strong impact on the GCM results.

6. The code availability

It seems the manuscript was submitted as the GMD "Development and technical paper". If it is correct, then "**The code should be made available, and a model availability paragraph must be included".** The code is, however, not available.

Once the code is available, I will demonstrate that its errors are much higher than reported in the manuscript.

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