Atmos. Chem. Phys. Discuss., 12, 25743-25789, 2012
www.atmos-chem-phys-discuss.net/12/25743/2012/
doi:10.5194/acpd-12-25743-2012

Phenomenology of convection-parameterization closure

J.-I. Yano, M. Bister, Z. Fuchs, L. Gerard, V. Phillips, S. Barkidija, and J.-M. Piriou

---

Interactive Discussion – Referee comments (26.11.2012)

The comments are structured in the following manner: i) overall opinion about the paper, ii) questions to the authors and iii) technical corrections.
Please read below.

i) Overall, I believe this paper is a very good review of the closure problem in the parameterizations of convection from the perspective of the science of the phenomena, because, once it is a review of a long-standing and well-known issue in the research community dedicated to investigate (and then aiming to improve) the representation of atmospheric convection in numerical models, I would expect to read the theory behind the question as well as how this particular problem has been addressed by other authors throughout time, and how the link between observations-theory-modeling is being established. My expectations were fulfilled. The reader has access to the definition of the problem, its context and its importance, the theory underlying this subject of research, and also the research done so far on this issue highlighting the different types of closures applied in the parameterization of convection and their interpretation. Many examples are given throughout the text, complementing the physical explanations of the process, easing the reading of the paper and settle the reader in the full context of the subject addressed in. In the present review, the authors focus on a particular key physical question whose answer is found in the Conclusions section.
The difference in the tropics and mid-latitudes physical mechanisms driving or controlling the convection is also addressed, which increase the value of this paper, once the scientific community tends to favor the research on one particular latitude band in spite of the other. Finally, the somehow recent posed question on high-resolution limit, that is, the dependency of the phenomena on higher resolution scales, when modeling is concerned, is also taken into consideration.
I consider the present review a very informative one, although somehow dense, directing the reader to the important aspects of the closure issue as well as the background theory, helping to achieve a good theoretical and practical understanding of the subject, and suggesting future research pathways.

ii) Questions/Comments to the authors.
I experienced some difficulties in understanding some statements done by the authors while reading through this paper. I would like to ask the authors to elaborate and/or develop them in a more clear and concise way.
They are listed below, *in italic*, followed by my comments.

1) *on page 25755, last paragraph, the authors state that "(…) CAPE didn't affect the linear dry or moist modes, but they were decayed by the CAPE closure".*
Are the first and the second half of the sentence referring to the CAPE closure?
I think this is not clear from the sentence, as it is, because in the first half the authors state that there is no consequence or effect of CAPE on the linear dry or moist modes, but instead if we considering CAPE closure, there is a consequence, an effect, related to the decaying of the dry or moist modes. Apparently, it looks like a contradiction, and somehow leads to confusion. I think that this statement may be altered into a more clear and powerful version of it.

2) *on page 25756, 2nd paragraph, where the WISHE mechanism is introduced*.
I am very confused about this paragraph, especially because the introduction of the WISHE mechanism. I don't see the reason for its introduction here, since it's not previously mentioned, as it would be expected if it was fundamental to the closure problem, and its importance isn't stressed out when we evaluate it from the perspective of developing instability, which is the guiding perspective of the present review.
I would like to suggest cutting out the mention to the WISHE mechanism, unless the authors provide a very good reason of its importance or contribution to the development of instabilities, that is, put it in the context of what the authors really want to address, referring it earlier in the text and be clear about how it acts when included in the closure-types analyzed in the paper.

3) *on page 25758, 4th paragraph, line 17.*
Are the operational experiments mentioned here, apparently, contradicting the observational results, in which some studies show a good correlation between convection and water vapor variables? (pages 25752, 25753 and 25754 corresponding to section 2.3) Do I understand this correctly?

4) *on page 25760, 2nd paragraph, line 10.*
The statement that starts in line #10 also leads me to experiencing some difficulties in interpreting it correctly. I think it needs to be restructured. To my understanding, the authors, referring to the Zhang (2002) proposed closure, state that the change in CAPE is due to two components: the changes in the free tropospheric environment and the changes in the boundary layer. However, only the changes in the free tropospheric environment are related to convection, as Zhang concluded. There is an inherent complexity in interpreting this sentence, in understanding its main aspect or reason. Also, since it's a review paper, I would like to know which changes in the free tropospheric environment (that are inferring changes in CAPE) is Zhang (2002) referring to and if they are connected with the physical variables mentioned and considered previously in the paper. I'm not expecting a long exposure on them, because I can read the Zhang's paper, but since the authors are considering and discussing the proposed variation in CAPE closure, it might be relevant to strengthening the discussion on this subject.

5) *on page 25766, last paragraph, line #21.*
This sentence is also not clear to me. The sentence starts 2 lines before, where the authors state that the "*cloud work function, A, is modified by a rate (…) defined in terms of a matrix Y, that characterizes the efficiency that convection type (…)*". What the authors are trying to say is that the matrix Y that characterizes the efficiency of a particular convection type in modifying the other convection type, given a specific cloud-base

mass-flux M? If my interpretation is correct, I suggest restructuring this sentence in order to make it more clear and concise. The way it is, it's not easy to access its full and real meaning.

6) *on the page 25769, last paragraph, line#25.*
The deep convective scheme used by the authors on their results includes a triggering similar to Kain and Fritsch scheme. Since the results obtained by the authors substantiate the argument presented, I think it's useful to provide the reader with more information about this scheme, without delve into it that much. For instance, no reference is given about which Kain and Fritsch paper (1990, 1993 or 2004?) the authors are referring to. Only when I accessed the References list, I saw that the authors refer to the 2004 paper, which contains an update to Kain and Fritsch first proposed scheme, so the reference is missing in the text. Also, giving the importance of the choice of the deep cumulus convection scheme for the results, together the closure option, I would like to see explained the reason(s) of choosing a triggering similar to the Kain and Fritsch scheme, and its influence (if any) in the results.

iii) Technical corrections and suggestions (marked with a "S").

1) page 25747, line #4 – "parameterization",
2) page 25748, line #6 – "adiabatic",
3) page 25750, line #18 – "models",
4) page 25753, line #11 – **S**: "good" instead of "nice",
5) page 25754, line #21 – **S**: delete "at all",
6) page 25757, lines #1,2 – **S**: "is balanced by" instead of "balances with",
7) page 25758, line #9 – missing "." at the end of the paragraph,
8) page 25758, line #23 - do the authors want to say "the drawbacks of" instead of just "drawback of"?, also delete "either".
9) page 25758, line #27 – "that are" instead of "that is", because there are several variables that aren't statistically parameterized properly,
10) page 25759, line #1 – **S**: "As a reminder" instead of "To repeat the point",
11) page 25761, line #11 – **S**: "Therefore," instead of "So,",
12) page 25761, line #15 – **S**: "Partly because" instead of just "Partly,",
13) page 25761, line #17 – **S**: "stronger" instead of "more",
14) page 25762, line #10 – "mesoscale",
15) page 25762, line #21 – **S**: "and" instead of using ";" because Bretherton et al. (2004) and Hohenegger and Bretherton (2011) are following Mapes's idea,
16) page 25762, line #24 - missing "." at the end of the paragraph,
17) page 25763, line #2 – **S**: substitution of "against CIN" by a more concise word, since no one is against the phenomenon, but argue about its approach,
18) page 25763, line #2 – "Here, the main points" instead of "Here, main points",
19) page 25763, footnote – "temperature" in the 1st line,
20) page 25764, line #1 – "cloud",
21) page 25766, line # 19 – missing "respectively" after "designated by lambda",
22) page 25768, line #27 – **S**: "greater" instead of "more",
23) page 25769, line #18 – "an increasing in moisture convergence",
24) page 25769, line #25 – "Kain and Fritsch (2004)",

25) page 25771, line #19 – **S**: "(…) give an evidence that the CAPE closure does not work well in the tropics",
26) page 25774, line #11 – " air parcel" instead of just "parcel",
27) page 25774, line #13 - "In the cases where (…)" without the ","
28) page 25775, line #12 – " The most disappointing of such conclusions (…)",
29) page 25775, line #15 – "a point of view of energetics of convection (…)",
30) page 25776, line #21 – "From a point of view that convections is a dynamical process (…)",
31) page 25782, line #7 – "atmospheric",
32) page 25783, line #31 – "advective".