

Interactive comment on “On the parameterisation of the urban atmospheric sublayer in meteorological models” by A. Baklanov et al.

Anonymous Referee #1

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It is my assessment that this paper does not contribute anything substantively new. Rather, the paper presents a summary of other papers that this group has completed without any cross-cutting synthesis. Unfortunately, the authors have not even taken this opportunity to harmonize notation within the paper. Moreover, it is clear when the authorship of sections changes. Thus, I do not think this should be published in its current form.

The authors need to: (a) use common notation throughout the paper, (b) reduce repetition, (c) make it clear what this paper contributes to the literature which is new over and above the individual papers that are cited, and (d) ensure that the English is of a good standard throughout (it is clear that someone has not gone through the whole

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paper with the eye to creating a consistent text).

Sections 4 and 5 need to be integrated into the earlier part of the paper. At the moment these sections appear to be “lifted” from other papers and detract from the earlier part of the paper by not having consistent notation, language, nor style that recognizes the earlier text.

The model evaluations are not presented in a rigorous or comparable way. So the independent sections add nothing further than the original papers from which the material is taken. For example, p12144 line 21 “very close” is given for the results of the model comparison with observations. Reference is then given to a figure, which is a time series with no statistics reported. Some text and tables which demonstrate the commonality and differences of model approaches, model performance, and model data requirements would be a significant contribution. Otherwise the reader is better served by reading the original papers that are cited in this paper. It is only when the reader gets to the conclusions that any cross comparison is presented.

P1210 Abstract - first sentence is very awkward. Abstract - currently there is nothing substantive indicated in the abstract

P12122 Lines 6- Oke et al 1999 is not a SEB scheme Lines 7 - Grimmond and Oke 2002 would be more appropriate to cite the 1999a paper Dupont 2001 - given part of the paper is about this model, I am uncertain why the paper reference is not used lines 17-19 : sentence unclear

p12126 - Line 9- this is a sensitivity test rather than a “verification”. It is not clear that this Figure actually adds anything. line 19 - any type of surface

p12131 line 17 - Figure 4 is not an “algorithm”.

P12133 section 3.1 - MOST should not be applied below the blending height; i.e. not just not below urban canopy. As the displacement height is typically less than the canopy height, this suggests that MOST is being applied below the height it is expected

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to be applicable.

P12134 - equations (1) & (2) - all terms are not defined P12135 - line 3 Zilitinkevich et al. (2005a) - this reference does not appear to have the correct year

Line 8 Zilitinkevich and Baklanov (2005) - this reference is not available from DMI The DMI web reference should be given for each of the DMI reports

Line 22—in this paper there are at least three different symbols/notation used for anthropogenic heat flux. Use one throughout, including the figures.

Notation for explaining symbols is not the same even within sections, compare definition of terms below eqn 3 and 4.

P121236 line 14 - the definition of heat storage flux where it includes “i.e. the constant flux layer” should be deleted. The definition is referring to the depth into the soil where there is no net heat transfer over the time period of interest not the atmosphere as is suggested by the “i.e.”

P12137 methods 4 and 5 are not explained in any detail.

P12138 - line 1 - equations 4 & 5 are referred to but the content is relevant to the remark

Section 4 - there are numerous errors in this section. The English needs to be tidied up. The text appears to have been cut and paste from the another document (I suspect a paper already published in this journal!) and so there are number of places it does not make sense and does not flow relative to the earlier sections. For example:

P12142 - line 28 - “for every mentioned surface” - this appears to have been dropped in here from another document and does not make sense. Change to “ for three surfaces”

P12143 -line 1 - no ‘s’ on radiation - line 4 specify the number of layers. - line 17 ‘higher’ not appropriate ‘the taller the building’ -line 21 - ‘larger’ - what is meant by this ‘wider’? -Line 25 - night - ‘nocturnal’ -Line 28- High - ‘tall’

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P12144 “The parameterisation has been tested on the city of Basel (Switzerland) and verified versus the BUBBLE experiment (Basel Urban Boundary Layer Experiment: Rotach et al., 2005).” Change to “The parameterisation has been compared with observations from the BUBBLE experiment (Basel Urban Boundary Layer Experiment: Rotach et al., 2005).”

Section 5 - here there is different notation from section 3. For example the storage heat flux. P12147 lines 25 -you may want to look at Voogt and Grimmond (2000), Kanda and Moriwaki (2002), Moriwaki and Kanda (2003). They determine values which are closer to 6-20 for an urban area.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 12119, 2005.

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