Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2019-176-RC2, 2019

Geoscientific Model Development Discussions

Interactive comment on "The Vertical City Weather Generator (VCWG v1.0.0)" *by* Mohsen Moradi et al.

Anonymous Referee #2

Received and published: 19 December 2019

General comments

The objective of this work, i.e. creating a detailed urban canopy model as an extension to simpler models (Bueno et al. model) is worthwile. The advantage of the proposed approach is that it can be used in standalone mode, not requiring a host mesoscale model, and thus constituting a very computationally efficient way of simulating urban climate.

However, I have several objections to the manuscript:

- the approach makes very strong assumptions (e.g. regarding the establishment of the vertical profiles); while sometimes this could work I am not convinced it does in this case, as I am not convinced by the validation results

- the manuscript contains dubious parameterizations (see detailed comments below)

- the manuscript contains odd data (e.g. extremely high saturation vapour pressure in Fig 5)

- I noticed odd model behaviour (city cooler than rural area during the night in Fig 7)

- too often clarity leaves to be desired; at several instances I couldn't figure out the meaning of a sentence, even after multiple readings. (examples in the detailed comments below)

A less important remark is perhaps that the authors do not refer to work by Erell and Williamson (2006), who were perhaps the first to implement the approach of the present manuscript, i.e. forcing an urban canopy model with rural data. (see https://rmets.onlinelibrary.wiley.com/doi/abs/10.1002/joc.1469)

Detailed comments

1. Introduction

p2 l10: (C. S. B. Grimmond, 2009) => (Grimmond, 2009)

p2 l31: "paucity of microscale models" - what about the widely used ENVI-met model?

p3 l25: Trees have a similar albedo than the urban surface, so have little potential for reducing the overall albedo. The authors should revise their statement on this or else demonstrate its validity.

p.4: The authors state that urban canopy models (UCMs) "are not coupled to the surrounding rural area". This is only partially correct, since UCMs generally are part of a mesoscale model and therefore are connected to the rural areas surrounding a city.

p.4 l28: 'the direction that turbulent...' => 'the direction in which turbulent...'

General comment: the Introduction is (too) long ; at the same time there are many references to rather old papers. The authors should consider reducing the length of the Introduction, preferably in those paragraphs that refer to the older papers.

2. Methodology

p6 l27: what is 'horizontal infrared radiation intensity' (I am puzzled by the 'horizontal' - is this about radiation on a horizontal surface?)

p6 l28: same question for 'diffuse horizontal radiation' is this perhaps radiation falling onto a horizontal plane?)

p6 l28: the Energy Plus Weather file is introduced a bit haphazardly, with just an Internet link as reference (while the data taken from it are crucially important for the method). I, for one, have no clue really what these data are, how good they are, what precisely is contained (hourly?, which location exactly?, ...). Why not employ, for instance, hourly ERA5 data from ECMWF? Or, for that matter, why not directly take vertical profiles from ERA5 or a similar data source?

p6 l30: how is the horizontal pressure gradient calculated from (I presume) a single pressure observation?

p7 l31: authors refer to Fig.4, but that is still several pages away => move this Figure closer to where it is referred to first

p8 l2: 'The atmospheric variation...': I am puzzled by the use of 'atmospheric'; do the authors perhaps mean to say 'the vertical variation...'?

p8 l7: why repeat the Internet link to the EPW? (appears on p.6 already)

p8 l25: 'In this study, the thermal stability condition is roughly approximated based on the available incoming solar radiation in the way that presence or absence of incoming solar radiation on the surface indicate unstable and stable conditions, respectively.' => I have no clue what this is supposed to mean concretely. I do understand that surface solar radiation affects stability, but from reading this sentence I can't imagine how this is done concretely in the model.

p9 eq4: the parameterization for u* appears very (too) simple: no effect of surface

roughness, no effect of atmospheric stability. I am sure that this expression works well for Guelph, Canada, but how well does this werk elsewhere?

p9 l6: 'When the surface is warmer than air, upward heat flux released into the atmosphere creates a thermally unstable condition.' Expressions like this are too obvious and belong rather to text books than scientific papers. As a general comment: the authors could considerably shorten their paper by removing statements like this one, which do not really contribute to the paper.

p9 l13: 'sensible heat flux from biogenic activity of vegetation' => what is this? I am puzzled by 'biogenic', which I know in the context of e.g. emissions of - biogenic - chemical compounds, but not when related to the sensible heat flux. Is this sensible energy released because of chemical activity associated with biogenic emissions? I suppose not, but that is what it looks like...

p9 eq7: cfr my remark on eq4: the parameterization of the transfer coeffient for the calculation of sensible heat flux appears too simple, not accounting for stability, roughness, ...

p9 l23: 'The rural model also outputs a horizontal pressure gradient based on friction velocity calculation...' => ????

p9 l25: what is the basis for the parameterisation for the pressure gradient (rho u*ˆ2 / Havg)? Please add a reference to justify this.

p9 l25: what is Havg? Later in the manuscript Havg represents average building height... (does the horizontal pressure gradient depend on the building height??)

p9 l27: the specific humidity does not vary with height in the model. This is a heavy assumption, but I can imagine that with only surface data there is no way to actually do otherwise. But the authors justify this by saying 'This assumption is valid so long as the water vapour pressure is less than the saturation water vapour pressure for a given altitude.' I disagree, as you can very well have a vertically variable profile of specific humidity even when 'the water vapour pressure is less than the saturation water vapour pressure'.

p11 eq15: how is waste heat (QHVAC) estimated?

3. Results and discussion

Fig.4: upper right quadrant: correct 'Hdomian' (suppose this should be Hdomain instead)

p15 l12: same remark as above regarding this parameterization of the pressure gradient

p16 l6: the authors (again) state that specific humidity can well be constant with heigth; in a situation with a non-zero surface latent heat flux this assumption will certainly not be correct

p17 Fig5: is it possible that the saturation vapour pressure (blue curve) is hugely overestimated? see eg https://en.wikipedia.org/wiki/Vapour_pressure_of_water for some values: at 25°C the saturation vapour pressure is approx 3.17 kPa, while Fig5 shows values around 20 kPa (which would imply temperatures of 60°C). Is it possible that the authors have made a conversion error between different units (mbar vs hPa / mbar vs kPa / ...)?

p20 Fig7: the caption should say which line corresponds to the model and which the observations. I can of course guess which line is either of these, but still it should be included.

p20 Fig7: I am not convinced at all that the graphs confirm a good model performance. The simulated profile (I assume the solid black line corresponds to model results - see remark above) deviates substantially from the observed one. Also, the observations (dash/dot line) almost appear not to change throughout the day, while the simulation result does vary between stable/unstable atmospheric profiles.

p20 Fig7: in the lower graph: the urban temperature goes below the rural value between (say) 0200 LST and 1000 LST. This is counter to anything I have seen in model results and observations. Surprisingly, in the the late afternoon / early night this is reversed (urban temperature > rural value). Could this issue be related to a spin-up effect? When consideringn Fig 15 (p30) I would think the negative UHI in the first night of the simulation might be a spin-up effect.

p21 Fig9: this validation for Q is not convincing

p22 Table2: using fractional bias or percentage error (cfr abstract) for temperature is not appropriate, because in that case the error depends on the units employed. A fractional bias on temperature expressed in °C or in K gives completely different outcomes. So, a statement such as 'the overal model bias on potential temperature is 5%' (abstract) has no meaning unless you specify the units employed, and even then it is better to avoid it.

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