Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-177-AC2, 2018

Geoscientific Model Development Discussions

Interactive comment on "The Air-temperature Response to Green/blue-infrastructure Evaluation Tool (TARGET v1.0): an efficient and user-friendly model of city cooling" *by* Ashley M. Broadbent et al.

Ashley M. Broadbent et al.

ash.broadbent.nz@gmail.com

Received and published: 19 December 2018

This paper presents a climate-service-oriented tool TARGET for diagnosing near surface air temperature based on urban energy balance. The reviewer strongly agrees the motivation of the work that the accessibility of urban climate models should be improved by providing end user-friendly tools with less demands for modelling expertise and specialized computing facilities. And the paper is well written with technical details clearly provided and results nicely presented. As such, the paper should be accepted after a minor revision. However, the reviewer has the following concerns about this

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Discussion paper

work and hope the authors can well address them:

We thank the referee for the constructive and thoughtful comments and agree there is a strong need for models like TARGET. We will respond to each comment below. In some cases, the original reviewer comments are broken down into sub-comments for clarity.

1a) Physics scheme of water surface: The choice of such a moderately complex lake model (Molina Martínez et al., 2006) should be justified, in particular considering OHM is used for other land surfaces, as this choice notably breaks the consistency in physics scheme for Qg.

Indeed, we wanted to use the OHM model (with force-restore) for all surfaces but we can not obtain good surface temperature results for water using OHM. We tested the parameters/modifications used in Ward et al. 2016 and still found substantial over-predictions of surface water temperature (over 10 °C) during the day.

We note that Ward et al. 2016 does not evaluate OHM performance for a 100% water surface and therefore does not truly demonstrate good model performance for water. Is the referee aware of such an evaluation in the literature?

If the OHM method in conjunction with force-restore can be shown to provide reliable estimates of water surface temperatures then we will implement that scheme in future model development.

As TARGET is a climate-service-oriented tool, we think that good model performance is more important than the consistency of physics schemes used. As such, we believe it is best to retain the lake model currently described in TARGET v1.0. We acknowledge in the manuscript the inconsistency of physics schemes used (P22L23). We will clarify, at the beginning of Section 2.6, the reasons we chose the lake model, and in the limitations (Section 6) we will re-emphasize the inconsistency with OHM.

1b) Also, the lake model used in TARGET is neither simple to guarantee calculation performance (e.g., vertical discretization is required to get water temperature profiles) nor sophisticated to consider the physical rigour (e.g., band-based absorption of solar radiation is omitted).

We respectfully disagree with the referee here. We believe the lake model is simple enough to “guarantee calculation performance” - we find minimal differences in model speed with the lake model switched on vs. off. Most of the surfaces are treated as 2 layers in TARGET - whereas the water surface is effectively treated as 4 layers - this does not substantially impact computation time.

However, the referee is correct, the model does neglect some physical process in water bodies. This is required to ensure model efficiency and simplicity. A judgment call was made to exclude some processes associated with water, including those mentioned by the referee. Extensive testing has found no unexpected behavior due to these omissions.

We will state all the physical processes currently omitted by the model in the paper and describe any known associated limitations. If the referees knows of anything specific that we have not mentioned in the manuscript please advise. We will happily include an acknowledgment of, and reference to, any associated limitations.

1c) The reviewer should point out that OHM can also be used for water surfaces to obtain Qg with easy adaptation (e.g., Ward et al. (2016)).

We do not believe (as mentioned above) that Ward et al. (2016) demonstrates that the OHM can be used accurately for water surfaces. Is the referee aware of a paper that demonstrates accurate Qg performance for a true water surface (i.e. 100% water)? Please advise.

2) Applicability for long-term applications: Although the limitation of TARGET in long-term applications has been attributed to that in OHM, it should be noted modelling advances in OHM (e.g., corrections in OHM coefficients (Ward et al., 2016), analytical

determination of OHM coefficients (Sun et al., 2017)) to address this issue should be mentioned and their potential in improving TARGET can be discussed.

We appreciate the reviewer pointing out these exciting opportunities for potential future work. If we can develop a version of TARGET that can be reliably used for long-term simulations that would be excellent.

For now, we do not want to encourage users to conduct long-term simulations with TARGET. First, we will need to do thorough testing and evaluations with long-term datasets. However, the additions the referee has mentioned will certainly be explored. We will note this as future work in the amended paper, and cite the papers that may be used for future improvements.

3) Code availability: The authors suggest the Java version of TARGET for performance reason, which interested the reviewer to review the python code in addition to the paper as more and more scientific models (e.g., Hamman et al. (2018), Monteiro et al. (2018)) are being published in Python for the easy accessibility (which TARGET claims as its key feature). After the code review, the reviewer noticed in the core calculation functions, the famous numpy is not well utilised to conduct heavy numerical computations. To make TARGET more accessible, the authors are very encouraged to improve the Python version for better performance and to distribute it via public repositories (e.g., PyPI).

Our intention is to offer versions of the code in both Python and Java. While we agree that the use of Java is less common and perhaps less familiar in model building, we do not believe that a Java version is less accessible than one in Python. Java, after all, is currently the most widely used computer language.

Java only requires the end user to have a Java runtime installed. All of the model's external dependencies and model's source code will be precompiled and packaged into a single Jar file. Python is not always as simple, requiring the correct Python (2 or 3) to be installed, with the external dependencies (i.e. NumPy, Netcdf, etc.), often requiring Anaconda to ensure the different versions do not conflict with other Python installations.

In our benchmarks, the Java version ran anywhere from 10-100x faster than the Python version. We agree that better use of NumPy would help optimize our Python code, as it makes array access more efficient and offers other performance gains, but we have no reason to believe based on our experimentation and research into the two languages that any optimized Python version will ever reach the performance of out of the box Java and its just in time (JIT) compiler. Achieving that magnitude of performance increase would likely require the use of tools such as Numba or Cython, requiring a level of user technical expertise that goes against our overall design principal of providing a model simple enough for anybody to use. Having said that, the performance of both versions fits well within our design goal of a modelling tool that is quick and efficient enough for widespread use. Both run quickly, just the Java version currently runs much faster.

Optimization and improvements of both versions of the model will always be an on-going goal. Once the article has been published and people start using the Github repositories, feedback or code enhancements will be encouraged and integrated into future versions. Any specific suggestion on how to improve numerical performance with Python would be welcomed.

Ongoing improvement of the Python and Java code will be carried out but this should not, in our opinion, delay the publication of the TARGET paper.

Minor Comments:

1) Please justify the assumption for combined resistance between roof and canyon in equation 16.

It is unclear to the reviewer how the influence of roofs can be exerted on canyons.

Thank you for the opportunity to clarify.

There is no consensus on how roofs might affect air temperature in the canyon. In reality, this interaction is dependent on building height, street width, upwind configuration, presence or absence of tall trees, wind speed and atmospheric stability, to name a few factors. Some urban canopy models assume roofs can be directly connected to the canyon air (e.g. Community land model urban [Oleson et al., 2010]) while others assume no direct interaction (e.g.single-layer urban canopy model [Kusaka, 2001] or TEB [Masson 2000]).

In a coupled urban canopy model, the interaction between the roof and the urban canopy occurs indirectly via the atmospheric model. However, TARGET does not represent a two-way-interaction between the roof surfaces and the above canyon air temperature (Tb). As such, without some direct connection between the roof and canyon, there would be no way for rooftop cooling interventions (e.g. cool/green roofs) to affect canyon air temperature in TARGET.

We will add these sentences to the amended manuscript: "We hypothesized that the heat transfer from roofs to the canyon air could be approximated by two resistances in series (the canyon-to-atmosphere resistance (ca) and surface to canyon resistance (cs)). The logic here is that resistance to heat transfer from the roof surface to the canyon should be greater than ca or cs independently. Through sensitivity testing, we were able to demonstrate that this assumption improves predicted canyon air temperature."

For most cases, we think this assumption is quite reasonable (especially given other urban canopy models assume the roof can directly interact with the canyon air via a single resistance). The impact of rooftops on near-surface air temperature becomes more uncertain for taller buildings. The uncertainties here point to the need for experimental fluid dynamics work to better ascertain these resistances, and TARGET can be improved as the theory develops.

2) Presentation:

a) Equation 16 is tediously long: simplify it. We will include a simplified version of this eq. In the amended m/s. Most likely a summation over “i”

b) Section 4.1.1 –> section 4.2. Amended

c) Cwatr’ in “list of symbols” are duplicated twice. Amended

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