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Interactive comment on "The Met Office Unified Model Global Atmosphere 3.0/3.1 and JULES Global Land 3.0/3.1 configurations" *by* D. N. Walters et al.

Anonymous Referee #2

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The manuscript summarises the development strategy and the main characteristics of the UK Met Office UM 3/3.1 atmospheric model, as well as the land surface configurations.

The manuscript is interesting and a useful read, albeit probably more so to the community that makes direct use of the UM.

The technical characteristics are detailed well, systematically and with abundance of references. I have found that the order in which the developments are presented is slightly odd, starting with 3.0, going to 3.1, then falling back to comparisons with 2.0. This does not flow well and ends up creating unnecessary repetition at times. A more

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logical organisation could make the manuscript shorter and easier to read, leaving more space for science results.

There is, in fact, very little science content in the manuscript, which make it less valuable than it could, possibly because: a) the objective seems to be mainly one of documenting the model formulation, to be referenced by future papers, leaving very little space at the end for results;

b) due to the adoption of formulation changes that are implemented as "packages", it is virtually impossible to attribute improvements in model performance to individual formulation changes. Most of what is left is: "version 2 did this, version 3 did that". The manuscript would be significantly more interesting (also to a wider community) if more results from intermediate development steps were reported, or cited (where a publication exists), so as to better stress which ones were the crucial changes and which were only minor.

c) the (understandably unavoidable) presentation of global/seasonal means and of model error improvement/reduction for such a small number of fields cannot support a discussion of mechanisms. The discussion of the errors, e.g. the impact of changes in the treatment of clouds, or the soil scheme, is superficial and somewhat speculative. One would expect, going beyond global means, 1-2 highlights, analysed in terms of processes.

WIth that, the only substantial issue I have with this manuscript, which I think must be addressed, is the seemingly contradictory intention of aiming for a seamless approach, but ending up with the 3.0 / 3.1 dichotomy. It is unclear, throughout the discussion, why certain changes are needed (e.g. lumping up tiles at the land surface, with a questionable methodology) in order to obtain better results in NWP mode. I have found this part of the manuscript quite poor and confusing: yes, results seem better, e.g. in Fig. 9, but is it for the right reasons and, if reasons are valid, does this not deny then the entire idea of having a single model for all applications ?

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If no clear rationale for the co-existence of these two versions (3 and 3.1) can be presented, I think that the entire discussion of 3.1 should be removed and left for a future publication, when reasons become clear.

Minor points:

- the discussion on page 1227 (sec. 3.6) is quite hard to follow. There is a typo on line 24 of that same page - the entire first paragraph of section 4 has really no content: it starts with "we believe" and then provides no useful information about model formulation. I would shorten it and leave space for the subsections.

Interactive comment on Geosci. Model Dev. Discuss., 4, 1213, 2011.