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Geoscientific Model Development Discussions

Interactive comment on "The BRIDGE HadCM3 family of climate models: HadCM3@Bristol v1.0" *by* Paul J. Valdes et al.

Anonymous Referee #2

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general comments

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This paper describes the (largely shared) configurations of a family of climate models as used in a significant research group at Bristol University. The central family member, HadCM3, is well-known and was originally described in papers (and a number of non-peer reviewed technical reports, still readily available) published almost 20 years ago, although the other variants are much less well-described in the available literature, as far as I am aware. Despite the rather elderly nature of the models in question, they are still used and useful, and have been developed and modified in ways that I think sufficiently justifies revisiting and clarifying their documentation in the way the authors have done here. Treating HadCM3 and its spectrum of (roughly) resolution-

based variants as a family whose commonality and differences are best described together in one paper is a good approach, I feel.

Whilst it is impossible to cover every aspect of the performance of a global climate model, this paper covers a usefully illustrative spread of material, and is uniformly clear and well written. I have a few comments on specific areas, as detailed below, but on the whole I think the paper could be published in largely its current form.

My main general concern is based in the fact that this is a model description paper with little authorial connection to, or real acknowledgement of, the people who originally developed, coded and made the model available - presumably mostly Met Office and UGAMP/CGAM staff in the 1990s. As far as acknowledgement goes, I'm not sure how best this could be done, but it does feel like a very significant nod should be made in that direction. Content-wise, it would be good to be a little clearer on precisely what the BRIDGE authors have themselves added over the MetOffice/CGAM provided code and mods, especially considering that the full documentation for the Gordon et al HadCM3 is still available and that the HadCM3-M1 configuration described here is described as being almost identical to it.

specific comments

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WM="worth mentioning"

page 1, line 17: "version 1.0" - is the intention to upgrade the version number for the whole set of configurations any time there is a bug-fix or change that may only affect one of them? Putting the reference simulation ids in a more prominent place might help with the version/configuration tracking

p2,l10: "though" seems extraneous

p2,l17: "UKESM" the acronym should be explained, and a source given for this information. Perhaps "HadGEM3" is meant?

p2,l29: "Roberts et al. in review" - reference to (currently) unavailable literature

p3,l9: there are a number of other relevant model description papers for FAMOUS - the original Jones et al '05 paper should be additionally cited here at a minimum

p3,l14: Space needed between "Gordon et al.(2000)" and "HadCM3"

p3,l17: "this in relatively poorly documented" - perhaps "is"?

p4,l7: since a point is made of the computational speed of HadCM3 et al over more modern models, some detail of the computational throughput/resource requirements of the different configurations would be useful, either here or elsewhere

p4,l16,117: "L" only refers to the ocean resolution, and "H" only to the atmosphere

p6,l15;p7,l29: WM - as a regular lat/lon grid is used in both components, Fourier filtering of higher wave-number dynamics is done in both models in certain latitude ranges.

p8,l6/p8,l25: WM - the rigid lid formulation requires the pre-specification of "islands" around which the barotropic circulation may occur. The standard MetOffice HadCM3, at least, does not allow mass transport through the Bering St because of this, which may affect the AMOC stability characteristics

p8,l30: WM - the virtual salinity flux is calculated using a globally constant reference salinity, which can distort the local response to the surface water forcing

p9,l30: despite what many generations of UM code and documentation has asserted, the soil hydrology in all versions of MOSES is apparently not derived from Clapp and Hornberger '78, but Brooks and Corey '64 (see eg footnote at http://jules-lsm.github.io/vn4.2/namelists/jules_soil.nml.html). The model still names everything with Clapp-Hornberger, so it would seem unhelpful to readers to start referring to Brooks and Corey when they won't find these names in the model, but this might be an opportunity to stop propagating the C-H misinformation.

p10,l4: I recall presentations by Valdes some years ago that appeared to show statis-

tically different climates from the "same" HadCM3 ported to different platforms. Am I misremebering, or has this issue been cleared up to the authors' satisfaction?

p11,l4: MOSES2.2 can be used in FAMOUS, although most published FAMOUS papers use the MOSES1 variant.

p11,l21: WM - MOSES2.2 has two modes of operation. One functions as described here (calculating the exchange for each tile, then averaging the fluxes), and the other aggregates the different tile properties together /before/ doing one calculation of the average flux (see Essery et al 03). I assume the authors use the first mode - FAMOUS (eg the Williams et al '13 they cite) uses the second mode

p21-: from this point, some model names have an N or H appended (eg HadCM3-M2.2N) - I don't see where this is explained

p23,l12: Beware FAMOUS-M2.2! I believe new issues have very recently been found with the long-term drift of the climate of the model described in Williams et al (Smith, pers.comm) that may play into this sort of bias and require significant retuning. Do you know what the AMOC is doing in that run?

p25,l8: The section title is a little misleading, since this section is purely about heat transports. On the subject of pure TOA fluxes: I believe HadCM3 is known to have compensating biases in TOA short- and longwave fluxes, linked to known problems in the clouds. WM?

p27,l9: At shorter timescales, however, FAMOUS was found to have high levels of variability in the AMOC when compared to the RAPID data (Balan Sarojini et al, Ocean Science 2011)

p29,l17: the effectively shut Bering St may play a role here too

p29,l23-25: the end of this paragraph is phrased in rather too certain a manner concerning the reliability of the AMOC stability metrics presented eg in Liu et al'14 for my taste. It is too definite to claim that observations indicate that "the AMOC is in a

bistable regime". Theoretical metrics have been derived that suggest that this *may* be the case, but they are some way from being proved definitive.

p33,l1: Reading this section, the uninitiated might expect that they would be able to obtain and run the useful models described here themselves. In fact, they would have great difficulty even viewing the effective model code, given the web of libraries, patches and options (all, technically, supplied here) that the UM is built from. That is not the fault of the present authors, and neither is the fact that this well-known model effectively has no support or distribution mechanism. But I think that some warning should be given that the copious information supplied in this section is *not* really sufficient to run the model oneself, and perhaps contact details (maybe for Bryan Lawrence, Director of Models and Data at NCAS <bryan.lawrence@ncas.ac.uk>) for someone to start with if a reader really did want to get help installing or running the system for their own use?

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