

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

Anonymous Referee #1

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The purpose of this paper is to document the GCMs derived from HadCM3 that have been and will continue to be used by the authors for a wide range of climate research activities. This is an appropriate purpose for a GMD paper. The paper is clearly and well-written and serves the purpose. Its comparison of the results from various HadCM3-related configurations with each other and with CMIP5 models, to give an overview, is particularly useful. Although HadCM3 is quite an old model, it is still important. In the Web of Science, the paper of Gordon et al. (2000) on HadCM3 has been cited at least 100 times per year in the last decade.

I have first some general comments, then some specific ones. I'm sure that my comments can be addressed, if the editor agrees that they should be, but since they're quite extensive I'm classifying them as implying major revisions.

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General comments

(1) It's rather unusual for a paper to document a model that was mostly not developed by the authors, especially when other papers already exist about the model. Moreover the authors are not the sole users of this model. Of course the authors are aware of these points, and have cited papers other than theirs; I have suggested some more citations, and some minor rephrasing to avoid misinterpretation. In a way, this situation is like an open-source development (these models aren't open-source, although readily available). In view of this, the paper should focus on the aspects which BRIDGE specifically has contributed. For example, while the summaries of sects 2.1-2.4 of HadCM3 are clearly written, I feel that this much information is not needed, because it's essentially documentation of the original HadCM3, for which Pope et al., Gordon et al., and references therein should be cited. For the purpose of this paper, sect 2.5 is the important part, giving the differences of the BRIDGE HadCM3-M1 from the model of Gordon et al. These are stated to have very little effect.

(2) The nomenclature of the models could be confusing. The purpose of this paper is to describe models used at BRIDGE, but these have to be clearly related to or distinguished from other models, in the same family, that have been used at the Met Office and elsewhere.

HadCM3 and HadAM3 are models developed by the Hadley Centre. On p3 line 12-14, the authors write, "Since its introduction, HadCM3 (and related models) has undergone a substantial number of changes, bug fixes and adaptations, such that few of the versions of the model used now are truly identical to their original model description in Pope et al. (2000) and Gordon et al. (2000)." I think this statement is debatable. It is likely that Hadley Centre users of HadCM3, and at least some other users, would say that it is not *really* HadCM3 if it's different from the model described by the papers of 2000, as far as the AOGCM is concerned, except for bug-fixes and mods needed for porting. That is, HadCM3 has not been revised. Of course it's hard to decide what's a bug-fix versus a scientific change - that's why this is debatable.

It turns out that the BRIDGE HadCM3-M1 is practically identical to the model of Gordon et al. (2000) (section 2.5). This is useful to know, but I'd suggest that it means it would be clearer to call this model just "HadCM3" throughout the paper. However, we learn that the BRIDGE HadCM3L is a very different model from the earlier Met Office model HadCM3L (p14 line 32). (A reference should be given for the Met Office HadCM3L - I think it might have been first used though not named by Cox et al., Nature, 2000). Similarly, the BRIDGE HadAM3H is said to be different from HadAM3H as used by the Met Office (p14 line 24-25). It is therefore confusing to call the Met Office and BRIDGE models by the same names. At least in the context of this paper, I suggest that models from BRIDGE and the Met Office are given different names, so that the reader can be clear, when differences from HadCM3 are mentioned, which are the ones introduced at BRIDGE, and which are the same as in the Met Office version.

For HadRM3, is Hudson and Jones (a technical report on a particular application) the only reference that can be cited? Perhaps one of the early journal articles using this model describes it? How does the BRIDGE HadRM3 relate to HadRM3P of Jones et al. (2004) [Jones, R. G., Noguer, M., Hassell, D. C., Hudson, D., Wilson, S. S., Jenkins, G. J., and Mitchell, J. F. B.: Generating high resolution climate change scenarios using PRECIS, Met Office Hadley Centre, Exeter, UK, 40 pp., 2004] and to the HadRM3H and HadAM3H of Arnell et al. (JGR, 2003).

There are several published versions of FAMOUS. It would be useful if the authors could relate the BRIDGE version to those documented by Jones et al. (2005), Smith et al. (2008) and Smith (10.5194/gmd-5-269-2012, 2012). The Smith papers identify versions of FAMOUS by the run ID of the definitive UM basis file, so it would be useful to relate the runs detailed in Sect 7 to theirs. Also, FAMOUS is documented at www.famous.ac.uk, which could be cited.

(3) Effect of differences in configuration. It seems to me that there's not a very close connection between the model documentation of sect 2-4 and the results of sect 5. It would be valuable, wherever possible, to relate the differences in results to the dif-

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ferences in formulation. I appreciate that some of this is done, and that it's hard, but whatever more can be done would be useful, since it's the kind of information that would be relevant to deciding or understanding the choice of model for a particular purpose.

(4) Computational cost (i.e. speed) is an important consideration in the choice of model. If one had infinite computing resource, for example, one would probably not prefer FAMOUS to HadCM3. It would be informative to collect some numbers for the relative computational cost of the model e.g. in Table 1.

Specific comments

p1 line 8. I would delete "originally". Subsequent developments are smaller changes than the original development; HadCM3 is still essentially the same.

p1 line 9-10. "but is now largely being replaced by more recent models" strikes me as an odd thing to say. It's true that HadCM3 is now little used by the Hadley Centre, which mostly uses later Met Office models. Some other centres, including BRIDGE, use HadCM3, but these centres, including BRIDGE, use other and later models too. Most centres, including the Hadley Centre, routinely use a range of models, older and newer, for different purposes. For reasons the paper explains, there are purposes for which older models are more suitable and cannot be replaced by later ones. I would suggest deleting this phrase.

p1 line 10. This paper is about the models used at BRIDGE, but other places use HadCM3 in the Met Office version. This sentence and the next two could seem to imply that BRIDGE is responsible for development of these models. To avoid that implication, you could slightly rephrase e.g.

It continues to be used by various institutions, including by the BRIDGE (Bristol Research Initiative for the Dynamic Global Environment) research group at the University of Bristol, who have made adaptations over time to the base HadCM3 model. These adaptations mean that the original documentation is not entirely representative of the

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models used at BRIDGE, where several other configurations are in use which now differ from the originally described model versions.

p1 line 17. In the title and here you mention "version 1.0" for this suite of models, but you don't mention it again in the paper, which describes each of the configurations individually. Is it useful to assign a version number to the entire suite? If it is, it would be worth explaining what defines a version of the suite, and what would qualify as a new version.

p2 line 4. It would be worth spelling out that "model" is a general circulation model. Not all climate models are GCMs.

p2 line 6. The statement is correct that the HadCM3 family is in use at BRIDGE, but it could seem to imply that BRIDGE is the only group which uses HadCM3 etc. still. As the authors know, and have stated at p3 line 14, it has other active users too.

p2 line 7. While it's true that Gordon et al. (2000) describe developments to the ocean model, I would say that main achievement and interest of that paper is HadCM3 i.e. the coupled model. Thus I would suggest phrasing this slightly differently e.g. "HadAM3 (Pope et al., 2000). Together with improvements to the ocean GCM, this enabled the development of HadCM3 (Gordon et al., 2000), which was one of the first coupled atmosphere-ocean GCMs that did not require ...".

p2 line 9. Replace "was" with "has been", since it's still in use. It has been used for the past as well as the future. I think that at this point it would be appropriate to cite some of the major uses of HadCM3 that don't come up later, such as detection and attribution (Stott et al., Science, 2000), unforced variability (Collins et al., Clim Dyn, 2001), climate projection (Johns et al., Clim Dyn, 2003), uncertainty and constraints on projections (Stott and Kettleborough, Nature, 2002), decadal prediction (Smith et al., Science, 2007). I've made other suggestions later too.

p2 line 10. Why is "though" at the end of the sentence?

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p2 line 16. Reichler and Kim (BAMS, 2008) could be cited here as well.

p2 line 17. UKESM hasn't been introduced.

p2 line 24. Hewitt et al. (GRL, 2001) could be cited here as the first use of HadCM3 for a snapshot of palaeoclimate.

p2 line 26. Faster models are also useful for investigation of anthropogenic change on long timescales; HadCM3 has been used for that as well e.g. Gregory et al. (GRL, 2004), Ridley et al. (Clim Dyn, 2005).

p2 line 25. Although multi-centennial rather than multi-millennial, I think it would be relevant to cite Tett et al. (Clim Dyn, 2007) as the first use of HadCM3 for a study of long-term past climate change. Among examples of more recent uses of HadCM3 for such a purpose, I would say that Schurer et al. (Nature Geosci, 2013) could be cited.

p2 line 27-29. What boundary conditions are meant here? I usually understand BCs as concerning forced climate change. Of course there are numerous earlier examples of climate-change experiments with HadCM3. Large initial-condition ensembles are a good application too, I agree, but I would say that a greater use of HadCM3 has been for parameter perturbation ensembles, the next point in this para, so maybe the order of these should be reversed.

p2 line 29-31. I'm unclear what distinction is being drawn between "multiple runs to explore the sensitivity" and "calculating probability density functions". Could these be merged? Murphy et al. (Nature, 2004, the first QUMP paper), and Stainforth et al. (Nature, 2005, the climateprediction.net paper) should be cited.

p3 line 9. Jones et al. (Clim Dyn, 2005) should be cited here as well, since it was the first publication of the FAMOUS AOGCM, and Smith (2012, 10.5194/gmd-5-269-2012), which documents the most recent versions.

p3 line 12-14. See general comment (2), on nomenclature. Since this paper doesn't intend to survey all users and variants of HadCM3, it might be better to move the

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statements in the latter part of this para, from line 17, to the start of the para, and thus begin by stating that BRIDGE has modified HadCM3 in various ways.

p3 line 29. At line 28 on p4, the authors explain that HadCM3-M1 is not exactly the model of Gordon et al. (2000). It would be useful to say that here, since I assumed that's what it was on first reading. See also general comment (2), on nomenclature.

p5 Table 1. Please give units for all quantities that aren't dimensionless. Some of these parameters are generally understandable, such as resolution and "no Iceland", but I suspect that many or most of them are not self-explanatory. While I appreciate the value in documentation, I think we have to consider what the purpose of the paper is. As far as most readers are concerned, the interest in these parameters will be their physical effect, and the consequences of changing them; if they are of sufficient interest to mention, these should be described in the text, with references to the literature of the schemes where appropriate. Writing down the values which appear in the configuration of these schemes is technical documentation, rather than scientific, so perhaps it's not needed in the paper, or maybe it would belong better in an appendix.

p6 line 8. This is not an accurate statement. The base code of UM4.5 is available at that URL, but the scientific definition of HadCM3 also depends on a lot of code mods. This is explained in Sect 7, but should be mentioned here too, or you could delete this statement and refer to Sect 7.

Sect 2.1-2.4. See general comment (1).

p14 line 32. See general comment (2) on nomenclature re HadCM3L.

p15 line 10. I'm not sure it's as simple as that. I think the introduction of the GM scheme is important in alleviating the need for flux adjustment.

p15 line 16 and Table 1. The coefficient being described is the vertical diffusivity - I would not call that a mixing rate. HadCM3 has a prescribed depth-dependent background vertical diffusivity and a Richardson-number-dependent part, which is important

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near the surface. In FAMOUS, the latter part is omitted. The mixed-layer scheme is distinct from the vertical diffusion scheme.

p15 line 25-26. As far as I know, FAMOUS uses the same equation of state (UNESCO) and diffusion scheme as HadCM3 (except as noted in the last point).

p16 line 24-25. See general comment (2) on nomenclature re HadAM3H.

p16 line 4 and Table 1. I don't think the mention of RSOL will be intelligible, unless you explain what this means physically.

p19 line 26. Since it was earlier concluded that the two HadCM3s are the same, maybe it would be clearer to say "the CMIP5 historical experiment of HadCM3 done at the Hadley Centre", to avoid implying it's a significantly different version. Can you give a reference for this experiment?

p21 Fig 3a caption, same comment, "our version of HadCM3-M1" implies that it differs significantly from the Hadley Centre version.

p29 para from line 21. The net meridional freshwater transport may not be a reliable indicator of bistability. Sijp (Clim Dyn, 10.1007/s00382-011-1249-0, 2012) shows that it's the derivative wrt AMOC that may be the determining factor. Hawkins et al. (GRL, 2011, 10.1029/2011GL047208) demonstrate AMOC bistability in FAMOUS.

p30 line 3. Please give a citation to someone it is known by.

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