

## ***Interactive comment on “Description and basic evaluation of simulated mean state, internal variability, and climate sensitivity in MIROC6” by Hiroaki Tatebe et al.***

### **Anonymous Referee #2**

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The authors describe in this manuscript version 6 of the climate model MIROC and its performance. Model description papers are useful to provide information that may be needed in future more science-oriented publications based on simulations with the respective model. Selecting the material for a model description paper is however a difficult task because it is clear that the information will always be insufficient to recreate the model from the description. I think that the authors present in general an appropriate selection of material. They only mention details of the model and its parameterizations where there are differences from the predecessor MIROC5, and they provide more or less typical evaluations of the simulated mean climatologies and variability based mostly on a pre-industrial control simulation. I appreciate that the authors

C1

describe the tuning procedure applied for arriving at the final model configuration. The presentation is in general clear and the use of language appropriate. I would recommend the editors to check in particular the use of articles, however. In general I would recommend publication of the article after introduction of minor revisions which I will list in the following.

L34 It's not clear to me what “directly resolved stratosphere” means. Could one resolve it indirectly?

Introduction: I would recommend to shorten the introduction. This is a paper for specialists who know about global warming, IPCC, and the purposes of climate models. It would be good to report which specific goals the MIROC6 development had but I would cut the general introduction.

L42ff “. . . has been already observed that . . . will drastically change” Rephrase if you want to keep this sentence.

L46 Not clear what “will increase” means e.g. for tropical cyclones. In size? Its number? Its strength?

L56ff Why are only the two most recent ARs mentioned (if one wants to mention them at all)?

L64 Is there such a consensus that “sophisticating . . . parameterizations . . . are necessary” “to reduce uncertainties . . . in climate projections”?

L82ff What means “K-1 model developers”

L116ff As before: How may improvement of parameterizations “may result in reducing uncertainty”?

L126ff The sentence on the “signal-to-noise ratio” is difficult to understand.

L139/140 I'd try to avoid terms like high-resolution” or “medium resolution” The notion of what is high, medium or low is very different among climate modelers and cer-

C2

tainly changes over time. L151 I suggest to add a sentence on how MIROC relates to MIROC-ESM (which is referred to in Section 3.2.2). In my understanding MIROC6 is a climate model that concentrates on the physical part of the Earth system and it would be useful to mention that because many of the models used in CMIPs these days include some component cycles.

Section 2: I'm missing some technical information in the model description. I guess that in particular time steps (atmosphere, ocean, coupling, exceptions for specific parameterizations) had to be changed in comparison to MIROC5.

Fig. 1: Do the marks indicate half levels or full levels or what else?

L164 Why is there a Table A1? I'd suggest to use a simple numbering for all tables.

L174 I would speak of "model top", not "TOA".

L184 Not clear what is meant with "dry air ... is insufficient".

L224 Remove "a".

L225 Not clear why there is reference to "future versions". Does the current model version use the described features or not?

L233 Tuning of gravity wave parameterizations. Often, the Hines parameterization is used with very simplified and globally homogeneous characteristics of the gravity waves at the launching levels? This may make it, however, difficult to tune as well the QBO and high-latitude circulations. In particular as this is a feature new to MIROC it would be useful to elaborate a bit more on the tuning of GW parameters.

L244ff Example of the SSNOWD parameterization: It is useful to mention that the new parameterization is "physically" based. But in general I would like to read what the motivation for introducing changes with respect to MIROC5, what the expectations were, and, later in the evaluation, if the expectations were met. This is done well for some of the changes (e.g. L271: "increased vertical layers have been adopted in order

C3

to .."), but less for others (e.g. the tripolar ocean grid mentioned at line 262). I would like to ask the authors to do this more consistently for the changes because it may help other modeling groups to judge if specific changes may be worthwhile to apply in other models or not.

L309 Only extinction coefficients would not allow to compute the radiative effects of aerosols.

L322 "These" instead of "This".

L328 I guess averages over these time periods are used?

L344 "land surface components are determined" sounds odd. I guess they are interpolated from some dataset. Please specify.

L350 "Reproducibility" of what?

L362 Here, the first tuning step for the coupled model is described. But there needs to be some procedure to specify tuning parameters in the component models, or not? Furthermore, I think it would be very useful for other modellers to be more specific about which parameters have been tuned to which effect. In some places this is described in acceptable detail, but in particular the first two sentences of this paragraph are very vague.

L376 Which "cooling effects" are meant, here? Aerosol-cloud effects as mentioned in the sentence before? Or total aerosol effects? I'd also prefer to speak of "radiative forcing" instead of cooling effects.

L402 Apparently global mean SAT is not a tuning goal. It might be useful to mention this, and also why not. Additionally I'd like to read a comment on the imbalance of about 1 W/m<sup>2</sup> that seems to exist in equilibrium. It seems like there is some artificial energy source in the model. Is there any knowledge where this originates from? Atmosphere, ocean, dynamical core, specific parameterizations? Is it known if this changes with the model state?

C4

Fig. 4 and corresponding text: Names for the TOA fluxes are confusing. What is called NET is actually the total net flux, while what is called OSR is the net SW flux.

L426 The sentence on the consideration of “global-mean values” for RMSE calculations is difficult to understand. Maybe provide a formula or clearer description on how the RMSE are calculated? Is that true for all RMSE in this manuscript? It would be good to mention in every caption of Figures where RMSE are presented how these values are calculated, i.e. in particular if a global or some other mean have been subtracted before calculation of the error. The OLR in Fig. 4 looks particularly confusing without such information because while the RMSE is smaller in MIROC6 than in MIROC5 one would guess otherwise from the color shading because of the dominance of red in the case of MIROC6.

L476 I accept that for many climate variables it may not be essential if the evaluation is done for a pre-industrial or present-day simulation. But for some it is crucial. The energy balance is such a case, because the total net TOA flux should be zero in equilibrium. One can't say that the imbalance in the models is consistent with some observed imbalance, because the latter is related to the system not being in balance currently. It is also necessary to provide a reference for the observed value.

Fig. 4 and others. Parts of this and other figures are very blurred. This should be improved for the final publication.

L512ff The region of the western tropical Pacific is singled out as a region of improvement in MIROC6. It should be mentioned that it seems that in other regions there is a clear worsening.

L518ff For the discussion of the upper stratospheric warm bias it also matters that present-day and pre-industrial are compared due to the known stratospheric cooling with increased GHGs and reduced ozone.

L519: Again, I would prefer to speak of model top or lid and not of TOA.

## C5

L522 It would be good to say that this is the stream function of zonal mean meridional winds and not of residual winds, I guess.

L524 Please rephrase this sentence.

L530 I guess the absorption of LW radiation plays a minor role compared to SW radiation.

L561 “extend”, not “extends”

L561f Not clear what “more active troposphere-stratosphere” interactions are supposed to mean (radiative, wave coupling, trace gas exchange?) and why the stream functions would indicate that.

L566ff There is no “remarkable improvement” in May. Furthermore, I suggest to avoid subjective terms like “remarkable”. Please check all the text.

L586 “into which cold an dense water forms” Please rephrase.

L609ff This is no sentence.

L612 The caption of Fig. 15 says only “temperature” while here you speak of “potential temperature”.

L614 Please rephrase “is risen”.

L623 “Better representation of cloud physics” would be “required” for what?

L638 Remove “to”.

L648 It would be useful to check if the historical simulation shows more realistic numbers in the comparison to [resent-day sea ice.

L649ff Is there any idea why Antarctic sea ice is strongly underestimated?

L655 Please specify what “sea level height” is presented in Fig. 18 and to which data it. Is compared in the text.

## C6

L676 “land surface variables” Actually I identify only one: snow cover.

L678 Again, global mean SST and SAT are variables for which the comparison of preindustrial simulations and present-day observations is misleading.

L695ff A prominent feature of SST and SAT biases is the strong warm bias close to Antarctica. This should be mentioned when discussing these variables.

L718ff It might be good to also mention the apparently missing features WIG and possibly EIG in the models. And what about anti-symmetric waves?

L727 Remove “and”.

L743 Remove “MIROC6”

L760 “become” Rephrase.

L778 MIROC-ESM should be introduced in the beginning (or here)

L778 “whic”

L787 Remove “that”

L787 What means “correlations . . . are not clear”? Insignificant? Small?

L793f SSWs are only a typical feature of the NH stratosphere.

Fig. 24 and its discussion. I’d find it helpful to add Figures for January to make clear also the deficiencies of the model.

L807ff One can’t evaluate the polar night jet in Fig. 7e because it shows annual means. It might actually be an option to add some seasonal wind fields in Fig. 7. The paper has many figures anyhow, so I wouldn’t mind adding a few more. Additionally there is a problem of chicken and egg with the wave-mean flow interaction mentioned here.

L846 Rephrase “existence depths”.

L885 “SLP anomalies are larger and better represented in MIROC6” The maximum is

C7

deeper, but otherwise I find it hard to judge which of the two models is better.

L889 I’d avoid words like “excessively” which are subjective statements.

L918ff In IPCC AR5 and also the paper by Andrews et al. (2012) which is cited, here, climate sensitivity and forcing are calculated from 150 years of the 4xCO2 simulation, not 20 years. I would suggest to follow this 150-year standard to ensure comparability. Some models show clear non-linearities during this period. It seems like the effect is relatively small in MIROC5, but this would need to be confirmed for MIROC6.

Tables 2 and 3: It would be convenient for the reader to combine the tables.

L966,979 Again, please avoid “remarkably”.

L999 Why would ECS quantify uncertainty?

L1028f It’s true that the hiatus is sometimes associated with the IPO, but there are plenty of other attempts to explain it and even arguments that the real reason maybe unidentifiable. So I’d suggest to not only mention the IPO.

L1031ff I don’t understand this sentence. I agree that the simulated hiatus could be spurious, but the argument of the ensemble mean wouldn’t support this.

L1055 Should a new paragraph start here?

L1068ff I have no idea why the final sentence suddenly makes a statement concerning component cycles which were not at all mentioned anywhere else in the text.

L1074 I don’t know what the policy of GMD is concerning the availability of primary data (which I think should be the code of the model and all input data needed to redo the experiments), but I find it problematic that the code is only available under the condition of “collaborative research”. As mentioned in my initial statement, a model description is necessarily incomplete. It can only be completed by the model code.

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C9