Review comments in black.

Responses in green.

Review of The Canadian Earth System Model version 5 (CanESM5.0.3) by Neil et al. for GMD.

The paper describes the last version of CanESM. The main goal is to provide a reference for people who will analyse CMIP6 model outputs. Like all model description paper, there is a conflict between the need to be as comprehensive as possible, while keeping a reasonable paper length. In this point of view, the paper achieves a good and relevant compromise. It includes a short description of the model components, with all references for the reader who wants to go further in details. It contains a classical choice of model diagnostic to evaluate the model climate. It contains also informations, of the model sensitivity to standardised scenarios of CO2 increase. This content nicely matches the main objective of the paper.

I really appreciated the honesty of the authors, particularly when they described the consequence of the bug in p1 version, corrected in the p2 version. The paper also includes information about model quality control and performance. This is not of interest for CMIP6 data users. But model developers will find useful information on the way other teams work. Information that is hardly presented elsewhere. The general organisation of the paper is good. As non-native English speaker, I won't comment the quality of the syntax. I found very few typos, and thanks the authors for their careful proof reading. The main weakness of the paper is that the model is assessed in comparison with data and with the previous version of the model. There is no comparison with the CMIP5 models. This is not a major concern, as the model outputs will be used during CMIP6, and CamESM will be compared to CMIP5 and CMIP6 model in the next months by other authors. To sum up, this is a very good paper that perfectly fits the reason why GMD has been created for. I think it can be published after a few technical corrections.

We thank the reviewer for their constructive comments.

Major concern

Line 351. I do not understand how "version control, run isolation, strict checking and logging" can insure that the climate is reproducible. I agree that up to now, nobody has observed that lack of bit identical reproducibility in an ESM can drive to a different climate. But we have also theories of deterministic chaos showing that this is possible.

The system described enables us to ensure that we can re-run precisely the same code, in the same way as an original run. On a given machine, the results are bit identical. In some cases, moving across machines (e.g. our current migration from Cray XC40 to Cray XC50) also allows us to maintain bit identity. Hence these runs are precisely numerically reproducible.

Migrations to a different architecture or compiler might result in a bit pattern change. In this case our expectation (and experience) is that the climate will remain the same, but the realization of internal variability will be different. In this sense the run is reproducible in that we are interested in climate not weather. We accept that it is theoretically possible that mulitple-equilibria exist within the model in general. However, within fully coupled modelling this has essentially never been observed for a modern-day like climate, as the reviewer states. The only mulitple-equilibria we know of in complex GCMs involve radical mean state / forcing changes such as under extensive glaciation. There is no evidence we know of that bit-scale changes can lead to a different climate state.

Indeed, we note that different initial condition realizations of the model - with a similar type of small perturbation - result in the same climate. The concept that there are infinitely many possible realizations with the same climate (rather than different equilibrium climates) is also widely employed in coupled modelling and international exercises such as CMIP, which make extensive use of initial condition ensembles. Thousands of such initial condition simulations have been conducted, without the appearance of different equilibrium climates, as far as we know. It is impossible to ever prove that bit-induced multiple equilibria do not exist - but the extensive number of previous simulations are evidence that bit-induced multiple equilibria are exceedingly

unlikely. For these reasons we are confident that the CanESM5 climate is robust to bit-pattern changes.

Minor concerns

Line 125 and following. Is there some specific representation of the urbanised areas ?

There is no explicit model for urban areas. Urban areas are represented/parameterized by higher albedo for visible light and lower albedo for near infrared radiation than for natural vegetation. The roughness length over urban areas is higher than that for crops and grasslands but lower than for trees.

Line 151 and following. The melt water of the glaciers goes to the runoff scheme. How did you design a 'river' routing scheme for the ice sheets ? (from slopes ?)

There is not a dedicated runoff scheme for ice sheets. When the ice melts, the liquid meltwater is treated like runoff and the river routing scheme carries it down stream to the nearest ocean grid cell. This is do-able since river flow directions are specified over glacial cells as well based on their topography. Please see Figure 1 of Arora and Boer (1999).

Line 162. For the NEMO TKE scheme, a better reference than Gaspar et al. 1990 could be Blanke, B. and Delecluse, P.: Variability of the Tropical Atlantic Ocean simulated by a general circulation model with two mixed layer physics, Journal of Physical Oceanography, 23, 1363–1388, 1993.

This reference has been inserted.

Line 218. What is the computing coast of CMOC and CanOE compared to NEMO dynamics and to LIM2 ? It is significant or not ?

Yes it is significant. Turning on CMOC reduces model throughput by a factor of 2, relative to the physical model. Turning on CanOE reduces throughput by a factor of 4 relative to the physical model.

Line 777. Bentson et al. 2013 is not in the bibliography. Probably a typo, for Bentsen.

Corrected.

Line 809. Mathews et al. 2009 is not in the bibliography. Probably a typo, for Matthews.

Corrected.

Line 840. What is "Global Mean Screen Temperature" ? GMST generally stands for Global Mean Surface Temperature.

Changed “Screen” to “Surface”.

Line 1335. Year of the paper is embedded in the URL of the DOI.

Fixed.