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Interactive comment on "Isca, v1.0: A Framework for the Global Modelling of the Atmospheres of Earth and Other Planets at Varying Levels of Complexity" *by* Geoffrey K. Vallis et al.

Anonymous Referee #1

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As its title suggests, this manuscript describes a modeling framework, based around a well established dynamical core, for simulating the 3D time-dependent circulation of Earth-like atmospheres over a wide range of conditions and with varying degrees of sophistication. The motivation is well described as seeking a traceable hierarchy of modeling tools that range from highly idealized, hypothesis-testing models all the way up to quantitatively realistic simulation models that can be compared directly with observations - at least for the Earth itself.

Although not unique, this looks to be a potentially useful set of modeling tools that has been designed to be relatively easy to install and use on a wide variety of platforms,

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and yet is based on some well established and well regarded models derived from the Princeton GFDL FMS model suite. An attractive feature is the availability of a set of Python programs, presumably designed and written by the Exeter group of post-docs and students, that can be used to configure the model, set its parameters and run it. This should make it accessible to relatively inexperienced users.

The manuscript itself describes the motivation and model formulation reasonably thoroughly and clearly, and includes a useful set of examples to illustrate what the model framework is capable of. My detailed comments below are mainly to seek clarification and to note some minor cosmetic errors and clumsy phrasing. But once these are corrected and addressed, the paper should be ready to be published.

The main substantive comment is that, although almost all the relevant parameterizations are described and discussed, I was surprised to see nothing explicitly mentioned about implementing topography, other than to say that it is included. This would normally be considered as one of the properties of the land surface that needs to be specified alongside the other surface boundary conditions, and perhaps should be included in an extra subsection in Section 5?

I was also somewhat less than convinced at the simulations of both Jupiter-like and Venus-like planets, neither of which looks more than superficially like their Solar System counterparts (the “Jupiter-esque” jets seem too weak and without an equatorial jet, for example, unlike the Schneider & Liu model comparators mentioned in the text, let alone observations). The Venus-like case also only seems to exhibit tropical super-rotation at very high altitude, near the model lid, which differs markedly from Venus itself. But perhaps this is expecting too much of a very idealized model! However, at a rotation speed 1/20 that of the Earth, wouldn’t the comparison be expected to look closer to Titan than Venus?

P.1 line 4 There is more than one “Geophysical Fluid Dynamics Laboratory” in the world(!), so this should specify that you are referring to the one in Princeton, USA.

P5 line 4 Not very satisfactory to refer to a paper still “in preparation” (Paterson & Vallis 2017).

P.5 line 16 Eq (2) This presumably only applies in the troposphere, as the optically thin stratosphere would become isothermal in radiative equilibrium. This expression is presumably a linearized approximation, which should be made clearer.

P.5 line 21 Should this be referring to equation (2) not (1)? Also delete “the” on line 22.

P.7 line 7 “the a” should be one or the other?

P.9 line 5 Is it really the case that the full RRTM cannot cope with more extreme conditions? Can you not recalibrate or extend the k-coefficients to adjust for more extreme situations?

P.10 Section 5 - An absence of discussion of topography here seems surprising. Perhaps add another subsection?

P.16 line 19 & ff Wording here is rather clumsy. Something like “where \sigma_b represents the lowest level at which the drag is applied, \sigma_t is the uppermost level and. . ...” would be better.

P.17 and Figure 9 and associated text. The vorticity is perhaps not the easiest field to interpret quantitatively for the “Jupiter-esque” case. Perhaps also show a latitude-height section of zonal velocity?

P.17 and Figure 9 - Why are the jets closed at $\sim$0.1 bars in the Venusian case? Are they also closed for p_s = 7.9 and 62 bar? Showing u with a vertical scale that is linear in pressure is not very clear. . ..perhaps linear in log p or height would be better? How many vertical levels in each case?

P.18 line 2 Parentheses around references needs attention.

P.20 section 9.1 Are there any plans to make post-processing routines more widely available? Perhaps via a user forum?

P.22 line 2-3 A word seems to be missing here to make sense.

P.22 line 10 “it is TO provide a means” (word missing?).

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