This manuscript documents two simulations from a new Mars global circulation model, ported from the UK Met Unified Model. The addition of new, independent global models is welcome in the community and should always be encouraged. Its development will increase the capabilities of the modeling community. In advocating for the development of a new Mars model, the manuscript also makes a strong case for the need for a Mars Atmosphere Inter-comparison Project. CMIP has been extremely successful for the assessment of future climates for Earth, and a MAIP might hold similar promise for Mars.

Generally, the model proves capable in simulating many of the large-scale features of the Martian climate. The model reproduces a dust cycle, which bears a reasonably good resemblance to that of Mars, without the need for forcing the simulation to observations. This is an impressive advancement. The manuscript is well-written and organized; however, a couple of discussion points are neglected, and most importantly, a key process is not included in the model. See main comments below and annotated PDF for minor comments.

Major comments:
1.) Most critical of the major comments: I understand that porting a terrestrial model to Mars is a substantial undertaking, but a Mars model lacking the CO2 cycle—and therefore having surfaces pressure being up to 20% too large for a given grid cell—seems like a massive (literally and figuratively) potential source of error. The authors recognize the need for a CO2 cycle on lines 521–531. A non-exhaustive list of the potential problems in simulating a realistic Martian climate without the CO2 cycle include: 1.) incorrect tidal amplitudes, since a given radiative forcing will, for a large part of the year according to Fig. 5, be working on more mass than actually exists. Because the tides are so important for closure of, for example, momentum budgets, getting this wrong makes the entirety of the presented results less robust. 2.) The reality of the radiative influence of dust may also suffer. If X amount of dust is lifted, the mixing ratio of X amount of dust would be less than reality if there was an incorrectly excessive amount of non-dust mass in the atmosphere. This comparatively thinner dust layer may not necessarily change the surface temperature because the total extinction would be nearly the same, but it might change...
the vertical temperature profile based on changed vertical distribution of dust. 3.) Similar arguments could be made on the impact spurious mass might have on various wave modes, but without running experiments, it is hard to know the non-linear changes on a model never adapted for Mars before, which is the point. 4.) Finally, the CO2 cycle is associated with a flow of the atmosphere from one pole to the other as the polar caps sublime and deposit. This flow, while comparatively smaller the zonal winds of the Hadley cell itself, is still missing, and with it one of the important potential mechanisms for dust lifting in the high latitudes.

While overall, the simulated climate looks reasonable; as plotted in sigma coordinates, the effect of an incorrect surface pressure is obviated. I am left to wonder if the model would look even better if this neglected process were included; conversely, I am concerned that not including this process is hiding other issues that are not yet apparent. I would strongly encourage the authors to investigate the feasibility of incorporating the CO2 cycle in the present version rather than saving for a future manuscript. At least, a simplified parameterization as noted used by other models around line 525, could be attempted. The process by forcing the atmospheric mass to a prescribed surface pressure or enforcing mass sources/sinks as need in the poles at the appropriate times of year might be sufficient.

II.) I missed a discussion on dust lifting. How is the process parameterized? How is the surface dust reservoir calculated? There is a brief mention in the results section on Line 320 that the UM calculates reservoirs and the horizontal motion, but this warrants a more complete description in the methods. Only on line 449, the fact that the dust reservoir is infinite is finally mentioned. This all needs to be organized into a specific section in the methods. It is impressive, as noted around Line 340, that a dust cycle is reproduced in the model without forcing, but it is difficult to assess how robust the cycle is without knowing how lifting is parameterized. One particular detail that appears contrary to the observed dust cycle is that month "9" of Ls~270 should have reduced dust MMR than months 6 or 12 (Montabone et al., 2015), but that is not the case.

III.) Is there any sensitivity to model resolution (predominantly horizontal but vertical as well)?

IV.) The results and discussion focused on the zonal-mean structure. This is an excellent way to put the bulk climate into context but is not the full picture. As the goal is a comparison of the UM MCGM to the PCM, at least some investigation of the non-zonal mean structure is warranted. The addition of at least a few plots showing the column optical depth for each season on a lat/lon figure would show that dust is being transported within the circulation in a realistic way. Similarly, plots of the surface temperature and pressure would demonstrate how poorly or how well the model manages to capture the true climate without a CO2 cycle (Major comment I).

Please also note the supplement to this comment: