Reviewer Comments on Ch 24 “The SCM Concept and Creation of Forcing Datasets” by Minghua Zhang and colleagues

Reviewer A:

Review of “The SCM Concept and Creation of Forcing Datasets” by Minghua Zhang, R. C. J. Somerville, and S. Xie.

The authors review the use of SCMs and forcing datasets in the context as it has been used in the ARM program. This is done well in a concise manner. I only have minor comments on the manuscript.

The use of CRMs or LES as point of comparison to SCMs and as driven by the identical forcing datasets as SCMs could be made more prominent in the present manuscript. In particular, I think in Section 1.1, they should be discussed saying that when they’re driven by the same forcing that they can be used as a source of information on some quantities that observations cannot yet give.

done

I also think that there should be some more explicit reference to the use of SCM by GCSS/GASS – rather than only references to the GCSS/GASS cases that happen to use ARM datasets. In particular, it would be worth mentioning that one can create forcing datasets focused on a particular sub-component of the atmosphere – as was done in numerous boundary layer cases of the former GCSS boundary layer cloud working group (e.g. BOMEX, ATEX, DYCOMS, etc.). The forcing for these cases can be idealized from the observations, and indeed one of these cases is the ARM Shallow Cumulus case (used in the Lenderink and Suselj references of this paper) – which used the constrained variational analysis as only one part of a forcing assembled from different sources of information.

done

Finally, it would be worth mentioning somewhere that SCMs with prescribed forcing have limitations. For example, SCMs/CRMs driven in the way described here are somewhat less appropriate for deep convection cases when the convection propagates into the domain or middle-latitude cyclones with differential advection, whereas they are more appropriate for boundary layer cloud conditions under conditions of subsidence, where the break between the large-scale and small-scale is very clear. One might also mention the use of the weak-temperature gradient SCM/CRM modeling of tropical convection (e.g. Sobel et al. JAS 2001, Zhu and Sobel QJ 2012) to get around problems of not having interactive large-scale forcing.

done

Detailed Minor comments

Line 10. “The chapter also” is an incomplete sentence. done

Line 76. Lee et al. is not in the reference list. done

Lines 86 – 88 and Lines 93 – 96 say the same thing and thus are redundant. done

Lines 102 – 104. The sentence beginning with “Because” is not complete. don

Line 118. “vapor” not “vaper”. done

Line 165. I would say “reduced” not “removed”. done

Line 211. “Barnes” not “Barns” done

Line 243. The term with *ql* needs to be multiplied by *L* in order for the units to be consistent. This mistake was also present in Zhang et al. 2001 done. thanks

Line 283. “are” not “ar” done

Line 292. “Hume” not “Humme”; “Davies” not “Davis” removed

Lines 301-309. This might be slightly rewritten to indicate that these boundary sites and wind profilers have not and are not always available. For example, ARM decommissioned the boundary facilities in 2010. I’m also not sure of the current status of the NOAA wind profilers. done

Line 334. I think that currently MWR is only available at the Central Facility and that the other stations to which you refer were at decommissioned boundary facilities. Done. Thank you!

Line 377. MC3E was in “2011” not “2012” done. Thank you!

Line 408. Guichard is not in the reference list. Done

Line 431. Lenderink is not in the reference list. done

Line 440-441. This is not a complete sentence. done

Line 443. For MPACE, only the Morrison paper used the constrained variational analysis derived forcings. The Klein paper created their own forcing from ECMWF analysis data. done

Line 459. Evaluation should also include observations – not just CRMs and LES. Done, thank you!

Reviewer B

This is a well-written chapter by people who know this subject the best. It should be accessible to the average reader of the monograph, but it also provides useful information for readers who are more knowledgeable about ARM. It could be published as is, after correction of some typos I list below. I have only one substantive comment:

Two topics that are not discussed, which I think deserve mention, are (a) the limitations of SCMs, and (b) the consequences of using the constrained variational analysis forcing for what can and cannot be learned.

Regarding (a): SCMs have become an invaluable tool for evaluating the effects of different parameterization assumptions (and by the way, perhaps the contribution of SCMs and ARM to GCSS - now GASS - should be mentioned somewhere). However, we have found out over the years that they are not perfect predictors of what one will experience when one tries to port one's insights from an SCM into the parent GCM, because of how feedbacks with the dynamics can sometimes change things. Done GCSS, GCM relevance limitation,

Regarding (b): What one can learn from an SCM using the constrained variational analysis forcing depends on how one uses it to drive the SCM. Many SCM studies just apply the forcing directly. In precipitating regimes, the observed precipitation is the strongest constraint on the variational analysis, resulting in an adjustment of the grid-scale vertical velocity to be consistent with the precipitation. Most SCMs, given that vertical velocity, will produce approximately the correct precipitation. Thus, an SCM run that way cannot provide information about the fidelity of a model's representation of precipitation processes. It can provide information on other things (clouds, heating and moistening profile errors, etc.), so it is useful, but for other reasons. I find that a large segment of the community still does not understand this. Done in the limitation section a new section is added

Another issue with using the forcing directly is that as model temperature and moisture errors build up, after a few days a given SCM can drift into a very different climate regime and is no longer useful as a diagnostic after that. In some cases, given the nonlinear behavior of some parameterizations, a small change in the initial condition can even cause the model to drift to a completely different state. This was noted by Hack and Pedretti (2000), which I see it is in the references but is not mentioned in the text. It should be. Thus, most SCM studies are done for a few days at best.

Done in middle section

One can use the forcing in a different way, applying it and then adding a relaxation term that adjusts the SCM T and q back to the observed values over a time scale of a few hours. With this relaxation approach, the SCM never drifts too far, and now since the SCM is decoupled a bit from the precipitation effect on the forcing omega, it becomes possible (1) to look at how well the SCM simulates precipitation, and (2) to run the SCM for longer periods of time, since it is more anchored to the observed state. It is no longer as good for diagnosing heating/drying errors (although the relaxation increments contain information about that). I think it would be very valuable to readers of this chapter to understand the different ways of using the forcing; some of this is discussed by Randall and Cripe (1999), but most readers won't be aware of that.

Done in limitation section

Minor edits:

Line 11: Should be "also highlights SCM results" or "also presents highlights of SCM results" or something along those lines reworded

Line 55: "realistic data" - there is no such thing. There are only analyses and reanalyses to initialize an NWP simulation. These are a blend of data, model parameters strongly constrained by the data and by basic constraints such as geostrophy, and other model parameters more constrained by the model parameterizations than by any data. reworded

Line 67: Computationally intensive, but not impossible, as large ensemble perturbed parameter studies in GCMs have demonstrated in recent years. Originally these were crowd-sourced, but now they can be run on any high-performance computing platform.

Lines 102-104: As written this is a clause, not a complete sentence. done

Lines 113-114: It would be good to mention a specific example; the obvious one is marine stratocumulus. Done. thanks

Lines 134-135: "more often" - I agree with this statement for LES models simulating boundary layer clouds, but I don't think many (any?) CRMs doing deep precipitating convection use moist conserved variables. So "sometimes" rather than "more often." Thank you!

Line 147: Should be "following"

Line 202: Should be "datasets" done

Line 211: Should be "Barnes" done

Line 292: Should be "Hume" removed

Line 425: "ne" should be "one" done

Line 441: End of sentence missing done

Line 443: Add "and" between the two cited papers; and their dates are 2009, not 2010, I believe. Removed the Klein reference because its forcing was constructed from ECMWF analysis

Line 450: Should be "Fridlind" done

Line 456: Should be "of models" done

Line 459: Should be "hypotheses"done

Editor

This chapter provides a nice history of the use of SCMs in the ARM program, and the creation of the forcing datasets used to drive these models. The explanation of the forcing dataset construction is very well done, and will be well received by the intended audience, for example. While the contribution is along the lines of what I was hoping to see, I have one major item I would like to suggest and several more minor thoughts:

Major: The main thing missing from this article was a sense of how, and perhaps a little more motivation on why, ARM decided that SCM modeling was the approach that was needed. This story wouldn’t be complete without some background on the discussions that were made in the early days of the program: what observations were needed, the trade-offs that were made to get these observations, the challenges that the program needed to overcome in order to achieve the successes that the chapter does a good job explaining. In short, why did ARM do as it did, and what were the decision points?

As an example: Part of this story, in my opinion, is the formation of the SGP boundary facilities (BFs), which provided information on the boundaries of the domain. But these facilities had more than just radiosondes: there were also MWRs, AERIs, ceilometers, and other instruments. Were these instruments all useful for the forcing datasets? What else would have been nice to have there, from an observational point of view, but was not done (or possible) because of budget?

Minor points:

I think that a good portion of the major point above can be alleviated by the incorporation of an introduction. We aren’t using abstracts in this monograph, so you can replace that (and the outline) with a significant introduction that sets the stage for the rest of the chapter. done

The sentence from line 102-104 starting “Because…” is not a complete sentence done

The section 1.6 is very heavily oriented to the SGP, as is much of the chapter. However, as was pointed out, these forcing datasets were created for the other sites too, including some AMF campaigns. What were some of the challenges that were experienced there, and how were they overcome?

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The continuous forcing dataset is a very unique contribution by the ARM program. I feel that it should be make more prominent – this could be done by taking that material and making it into its own section. Be sure to indicate why the program felt that this was a good thing to do!

done

Fig X1: be sure to indicate that this is the ARM SGP site. And “circles” is misspelled.

Pg 17, line 441 – this sentence seems to have lost something as the thought isn’t completed.