The paper "Monte-Carlo methods for NLTE spectral synthesis of Supernovae" by Ergon et al. is generally well written, and deserves publication. The authors have carefully described a new non-LTE and time-dependent Monte-Carlo code, and have undertaken numerous tests to check the validity of the code. They also discuss extensively a new technique of using Markov chains to decrease the computational effort. However, I do have several concerns that need to be addressed before the paper is acceptable for publication.

1.

Referee: In the introduction the authors make a number of references to MC modeling associated with SNe. However, the techniques of Lucy have been used in other areas, and some reference to this literature is warranted. For example, there is extensive work by Alex Carciofi on using MC techniques to study Be and B[e] disks.

ME: Fair point. We have added references to the work by Carcofio as well to that of Long and Knigge in the first paragraph in the introduction.

2.

Referee: I get a little confused regarding the MC approach and its connection to lambda iterations. Most MC modeling utilizes the Sobolev approximation, and this helps avoid issues of non-convergence in the lines. The time-dependent approach also helps to overcome problems with lambda iterations, since it takes time for regions of large (continuum) optical depth to communicate with other regions. In the static situation, ALL depths are effectively coupled. A lambda-iteration propagates information one optical depth at a time (in one sense like the MC technique), whereas for an ALO it is more like one grid point at a time.

ME: Both points are fair, although the latter is a bit subtle as it applies to each individual time-step and not necessarily to the time-dependent calculation as a whole. We have added a brief discussion of these points to the second paragraph of Section 2.1.

3.

Referee: In 3.1.1 the authors note that they sometimes adopt the assumption that I=S. This seems to have worked in the case they considered, but I believe there are situations where this may fail. In situations where S is dominated by a single species, the assumption I=S is equivalent to detailed balance which is known to be a bad approximation in some cases.

ME: ...

4.

Referee: I would like to see a discussion of the computation resources needed to run complex models.

ME: ...

5.

Referee: In Figure 17 they show the influence of using 2, 4 and 8 lambda iterations per time step. Of particular concern is the variation in the U band. From the figure it does not appear to have converged. To examine convergence the crucial quantity to be examined is the ratio of successive

corrections, not the absolute size. If the ratio is close to 1, many more iterations may be needed to achieve convergence. *The authors should examine their convergence criteria carefully. *

ME: We are not sure we understand what the Referee means. Examining the U-band lightcurves in Figure 17 it is quite clear that the ratio of successive corrections is decreasing drastically (in terms of U-band flux).

6.

Referee: Footnote 5 should be rewritten to more clearly explain what is meant. The agreement between CMFGEN and JEKYLL does not prove that the codes can accurately model SN ejecta, especially since similar atomic data was used in the test. However, it does provide a test of the radiation transport and non-LTE solution in two very different codes which use very different techniques. How well did the light curves agree?

ME: Fair point. We have added a clarification to the footnote.

7.

Referee: The authors note that JEKYLL uses the Sobolev approximation but CMFGEN does not. In general, I agree with the authors that the Sobolev approximation is unlikely to cause errors because of the large velocity gradients in the SN ejecta. However there is one potential case where the approximations used in the codes could lead to inaccurate answers. That occurs when the intrinsic profiles of two lines overlap, and strong scattering is occurring within one or both lines. The Sobolev approximation will not treat this interaction, while in CMFGEN the number of such interactions may be overestimated by the use of large microturbulent velocities.

ME: Fair point. We have added a clarification at the end of the first paragraph of Sect. 4.3.

8.

Referee: If I read the text correctly, it would be informative to add "Both codes use LTE estimates for the population and the ionization state of the gas." to Figure 3's caption.

ME: Fair point. We have added this to the caption of Fig. 3.

9.

Referee: Did the authors do a comparison between the results of ARTIS and JEKYLL using the "best approaches" that can be used by either code? Such a comparison would be informative, and more relevant to discussing the level of agreement/disagreement between the codes.

ME: That is certainly an interesing comparison, but quite a bit outside the scope of the paper. The main purpose of the comparisons is to show that the JEKYLL code works as intended, and in that sense the JEKYLL-ARTIS comparison in the paper is sufficient. However, the purpose is also to initiate a process of comparisons between the spectral codes used within the SN community, so we will continue to work on this topic and hope present more results in the future. The next step may be taken already this summer at a workshop in Tel-Aviv, when a number of codes (including JEKYLL, ARTIS, SUMO and CMFGEN) will be compared using a benchmark Type Ia SN model.

10.

Referee: In the SUMO /JEKYLL comparison do the authors have an explanation why the temperature and ionization are in relatively "poor agreement" in the H and H/N zones, particularly at early times?

ME: ...

11.

Referee: In Figure 11 they show a comparison of the bolometric light curves computed under three different assumptions. It would be nice to see the photometric behavior in different passbands for the same set of assumptions.

ME: ...

12.

Referee: The authors show in Figure 12 the light curves for Model 12 which was the best fit model found by J15 to SN 2011dh. Can the authors show the 2011dh data on the same plot, or is that the subject of another paper?

ME: Yes, that is the subject of Paper 2, so we do not want to show any comparisons with the 2011dh data here.

14.

Referee: In general, the tick marks on the axes are faint, and a little short. In many cases they could also add extra unlabelled tick-marks (e.g., Figures 12, 17).

ME: Fair point. We have made the tick marks thicker and longer, and added minor unlabelled tick marks to most Figures.

15.

Referee: On page 8, when discussing the diffusion approximation, the use of "outer boundary" may be a little confusing -- it is the outer boundary of the diffusion solver the inner boundary of the MC solver. Perhaps use "upper connecting boundary" or just "connecting boundary".

ME: Fair point. We have replaced "inner/outer boundary" with "connecting boundary" in Sect. 3.5.

16.

Referee: Please recheck all equations in the appendix for typos. I did not do a detailed check.

ME: Done. A few typos found and corrected.

17.

Referee: There appears to be only one reference in Section 3.3.4 (The Markov chain solution). I would think more references may be warranted. Markov chains have been used to do radiative transfer, but I am unsure as to whether the technique has been applied elsewhere in a manner similar to that described in this paper.

ME: ...