(Below the referees' comments are cited in normal font, and the author's response to them in *italic font*)

Referee #1

The author presents results of a detailed optimization/calibration process on an empirical melt-index based snowpack model used in Norway to simulate seasonal snowpack evolution based on daily temperature and precipitation input. The optimization process involved a FAST sensitivity analysis of the 13 model parameters and MCMC simulation of the two most influential parameters. The optimization process was found to correct significant SWE biases in the previous version of the model.

The paper is reasonably well-organized and presented but is essentially a tuning exercise of an empirically-based operational snowpack model, and it is unclear what original science contributions the paper makes over Saloranta (2012). It is also difficult to explain systematic errors within the statistical framework used by the author. Model errors come about from several sources: (1) errors and biases in the driving air temperature and precipitation fields, (2) estimation of snowfall amounts (precipitation phase problem), (3) inadequate treatment of physical snowpack processes in the temperature-index model such as snow aging and melt, and (4) physical processes not included in the model such as blowing snow.

I realize the author has his/her hands tied with respect to the snow model used. However, the scientific value of the paper would be significantly improved with a more physically-based approach for improving the snow mapping. For example, the Bayesian approach could be used to generate pdf's of SWE estimates at each grid point based on the error characteristics of the driving variables and model parameters. A recent paper by Harder and Pomeroy (2014 - "Hydrological model uncertainty due to precipitation-phase partitioning methods", H-P early release) found uncertainties related to precipitation phase of 160 mm in peak SWE in alpine environments. The pdf's of SWE/depth could be used to estimate a snow cover fraction in each grid cell..

Author's response: *While the operational seNorge snow map model was thoroughly described and evaluated in an earlier paper (Saloranta 2012), the main scientific contributions in the current paper are the introduction of systematic model analysis techniques (i.e. combination of model sensitivity analysis and Bayesian MCMC parameter estimation), which together with some new model algorithms and revisions have led to significant enhancements in the model performance and thus in the quality of the operational Norwegian snow maps. The improvements to the model-based snow mapping method, as well as the applied way of systematic model analysis, which is not yet very common in snow modelling, should in my opinion be of interest to the readers of The Cryosphere.*

I agree with Referee #1 on that it is difficult to precisely explain and apportion the uncertainty and model error to its main sources (as listed by referee#1 and also already mentioned in the current paper (p. 1991, from line 4)). However, in my opinion, this challenge is not especially related to the statistical MCMC method used, but is a general challenge for many types of models, as already pointed out in the paper (p. 1990, line 7, with reference to Renard et al. 2010). Although the apportionment of the parameter- and forcing data related uncertainty often cannot be well resolved, their joint contribution on the model output uncertainty can still be rather well quantified by the MCMC parameter estimation method. This is a major advantage compared to many other calibration methods in use, such as calibration by trial and error or basic Monte Carlo simulation.

I also agree with Referee #1 that the basic degree-day method is a simplification of the snow melt process. This basic method is therefore in the current paper already extended with the important solar-radiation-related term (p. 1978, from line 5) to make it somewhat more physically-based and to enhance the model performance. The mostly physically-based melt models in general prove their superiority at heavily instrumented research sites, but since our "mission" is to simulate and map snow conditions in all of the 334000 1x1 km grid cells across Norway, we must seriously consider also the quality, availability, representativeness and robustness of the meteorological forcing input data that we apply. A full set of meteorological forcing data (short- and longwave radiation, wind, humidity, pressure, in addition to temperature and precipitation) necessary for the mostly physically-based energy balance melt models is not yet available for all of Norway at suitable quality and spatial resolution. Moreover, the snow model intercomparison project results (Rutter et al. 2009, JGR, doi:10.1029/2008JD011063) as well as many other studies, have shown that on average simpler snow models (e.g. degree-day snow melt based models) can perform as well as more complicated and physically-based models, and that one has so far not been able to point out a "universally best set" of snow models. We, of course, continue developing, examining and evaluating the different snow processes that could be included in the seNorge snow model, in order to increase its performance.

I agree with Referee #1 that the posterior pdf's resulting from the MCMC simulation could be used to express uncertainty of e.g. SWE in the snow maps. This possibility was already mentioned in the current paper (p. 1990, lines 8-9). The operational implementation and utilization of this information is certainly a task for future work. The pdf's of mean grid cell SWE probably have a strong contribution from precipitation uncertainty (both amount and phase), while the sub-grid snow variability is also strongly dependent on e.g. the topography and wind conditions. Therefore, the pdf's of SWE cannot be quite straightforwardly used to estimate a snow cover fraction in each grid cell, as suggested by Referee #1. The new method introduced to the seNorge snow model for estimating the snow cover fraction in each grid cell is explained in the current paper's Sect. 2.1.3.

In the revised paper these very relevant points raised by Referee#1 will be taken into account and further emphasized, discussed and clarified along the lines discussed above.

Referee #2

The author shows in this paper a Monte Carlo based parameter estimation method for the seNorge snow model. The paper is in general good, but some parts should be improved. I have the following comments:

- The sections 3 and 4 are extremely dense and very hard to follow for a reader who has not a strong statistical background. Its reading should definitely be helped by more charts and tables, and clarity should be improved.

Author's response: *The paper will be revised accordingly by clarifying text and by adding a clarifying chart/table to the Sect. 3 and/or 4.*

- In section 2.1.2. the descriptive metamorphism is neither mentioned nor included in the model. It is actually an extremely important process during the days immediately after snowfall, when snow is still light, and a model that wants to simulate snow density has to include it. See Anderson (1976) and SNTHERM (1991).

Author's response: *The destructive metamorphism, which Referee #2 refers to, is in the current model version indirectly included via the density-dependent compaction rate (higher rate for lighter snow) as well as in the higher initial density for new snow in the more windy treeless grid cells. This will be briefly clarified and emphasized in the revised paper version.*

- The parameters f_s and T_{cor}, which are essential, are first mentioned in sections 2.2 and are not explained in the model description

Author's response: *The reason for this is that these parameters are connected to bias correction of the input forcing data, and so they are technically not parameters of the model itself. In the revised paper version, these parameters will be introduced already in Section 2.1 and their role will be further emphasized and clarified.*

- The Nash-Stucliffe index is first mentioned on page 1978, but explained only on page 1986

Author's response: *In the revised paper version, the Nash-Sutcliffe index will be explained already where it is first mentioned (page 1978).*