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

**Reviewer #1**

***General comments***

The paper presents a new modeling system to estimate the snow distribution within Norway. This kind of system would be really needed, especially when taking into account a terrain with difficult topography which means also problems in in situ (and also remote sensing) observations. The paper addresses a relevant scientific question whether it is possible to use a rather simple modeling system reliably, and under which circumstances the model works well and not so well, and why so.

Paper presents a novel modeling tool, but in my opinion the conclusion reached is that the system does not estimate the snow depth or SWE distribution that correctly, that it would be useful in applications needing more exact values of the snow cover. Nevertheless, comparing locations and years in relative terms seems to be more promising way to work with the modeling system. Paper also reaches the conclusion, that the snow model needs development and/or recalibration. It was clear according the paper that elevation and time of the winter affected the model performance. I would still like to see more thorough discussion about possible reasons for the not-so-good performance of the model, and discussion on which processes in the model will need to be developed.

***Author's response:*** *I suspect, that the main reason behind the biased model simulations of SWE is the overestimation of input precipitation. This hypothesis is supported by preliminary model calibration tests, as well as by the fact that the bias in SWE increases with increasing elevation, where the precipitation input values become more and more uncertain, since most of the meteorological stations used in interpolating the input precipitation fields are situated in the lowland areas. Moreover, adjusting/calibrating parameters in the density algorithm (within realistic values) and/or modifying the algorithm, e.g. by removing the $\Delta SD_1$ calculation step and including compaction processes in wet snow, may well enhance the performance of the simulated snow density. Also, development of a multi-layer model or using a higher temporal resolution (i.e. shorter than daily time step and diurnal variation in the input data) might contribute to better model performance. In the revised manuscript (ms.), the discussion on these potential sources of uncertainty, reasons for the "not-so-good" model fit as well as processes to be developed further in future model versions is extended somewhat, as suggested by Reviewer 1. However, it is difficult to conclude firmly on the reasons for the "not-so-good" performance of the model without testing a revised model in practice. As pointed out in the ms. (page 1355, line 28), it is possible that several different processes or combinations of parameters may explain the detected model biases, and therefore, a suitable calibration method (MCMC simulation suggested in the ms.) is recommended to more quantitatively evaluate the possible reasons behind the detected model biases.*

The quality of the paper is good – it is well structured and written, language is fluent. In any case, check the tense throughout the paper. Methods are clearly described, although some assumptions behind the parameterization are not explained. Observations and calculations are sufficiently described. Title and abstract are descriptive enough. Mathematical formulae, symbols, abbreviations, and units are correctly defined and used. Some more references to other models of snow distribution and structure could be added. Discussion would benefit also of references on some classic works on snow distribution and snow depth/SWE variability (listed in e.g. in Handbook of snow etc).

***Author's response:*** *In the revised ms. tense is checked and some more references to reviews of snow modelling history and studies on snow distribution, as well as to previous similar model approaches are added (Armstrong and Brun, 2008; Anderson, 1973; Lindstrøm et al. 1997; Schreider et al. 1997; Clark et al. 2011).*

***Specific comments***

In discussion on overestimation of SWE and density please make it clear which of these quantities are calculated first, and which only second, using the other quantities as input.

***Author's response:*** *SWE is calculated before density. This is now pointed out in the revised ms.*

Try to clarify which processes may contribute to overestimation/underestimation in which conditions – forest cover, weather type, homogenus or not homogenous grid cell. . .

***Author's response:*** *The current model classifies grid cells to those above and below the tree line. Currently only melt rates (degree-day coefficients) are affected by the forest cover in the model, i.e. they are set to lower values in the forest than in the open areas above treeline. The potential effects of weather type and of forest canopy on snow accumulation, sublimation and melting (in contrast to areas above treeline) and their inclusion in the seNorge snow model are now briefly mentioned in the revised ms.*

Did you compare the two data sets with each other – are there possible comparisons that could give new insights?

***Author's response:*** *As pointed out in the ms. (page 1346, line 15; page 1352, line 10), the two datasets are rather non-overlapping and quite different in their characteristics, so their comparison is not very straightforward.*

Your input data lacks daily variation in meteorological parameters. Could you comment how this may affect the simulation quality.

***Author's response:*** *See response to the first comment. We have actually recently planned to test the model with new input data estimated for every 3 hours. This should in principle increase model performance, but it is difficult to say whether the increase in performance is substantial before testing it in practice.*

More discussion on possible problems related to observations could be added.

***Author's response:*** *The beginning of section 4 (Discussion) is now extended in the revised ms., clarifying the effects of measurement uncertainty (relatively small) and the uncertainty in how well the spatially limited observations represent the mean snow conditions in a model grid cell.*

Overall comment on snow model – are there references to validation studies on snow density / densification etc. – I mean VIC and SNTHERM process model validations?

***Author's response:*** *The viscosity-based snow compaction formulation is commonly used in snow models, and is based on empirical laboratory or field studies of, among others, by Kojima (1967) and Navarre (1975). A reference to these early works is now given in the revised ms. In addition, preliminary model testing indicates that the second step in the compaction algorithm ($\Delta SD_1$) might be unnecessary, causing some of the model density overestimation. This is now also commented in the revised ms.*

Mapping of the snow cover could perhaps be validated also against some remote sensing products?

***Author's response:*** *This is a good suggestion, and we have plans for utilizing satellite-images for model evaluation in the future. Detailed enough images estimating the snow-covered area (and surface wetness of snow) exist and could be used to evaluate the large-scale extent of snow cover in the simulated snow maps in the future. However, remote sensing products at the moment do not provide adequately good information on SWE. Other potential snow map evaluation data sources (including satellite images) are now mentioned in the revised ms.*

***Technical corrections (line numbering refers to the printable version)***
p 1338, line 10 – "distribution of model fit" sounds strange p 1340, line 14 – can you say it is a rather good agreement? P 1341, line 9 "thoroughly spatiotemporally evaluate" is complicated wording Line 15 what does "lack of accurate absolute values of snow conditions" mean, use another wording Line 27 – word missing somewhere

P 1342, line 13. Threshold temperature Ts seems quite low – 1.2 has been used in some other applications. Line 21-22. Do the parameters fs, fr change from one grid cell to other? Are they experimental? How about Cm, is it experimental – it takes into account forest cover and latitude. Should other parameters be considered also?

P 1346 line 7. Please consider linguistically more correct naming for the Norvegian meteorological institute data series than "met.no-data". Line 19. Density is calculated from SWE and snow depth? So overestimation of modeled snow density is just a consequence of overestimation of SWE or underestimation of SD? Wordings "somewhat better suited" is complicated.

P 1347 line 7, method description complicated P 1348 line 10, why in February? P 1349 line 21, you did not use original density observations? P 1350, line 13, why so? Line 19 "almost equal dates" is a bit strange Lines 21-23, complicated sentence, please re-write. Line 30, kg/l is not SI-unit. P 1352, line 2, to explain – are included? Line 7, spatial natural variability – natural spatial variability P 1353 line 8-13. Sublimation of blowing snow and snow redistribution certainly have an effect on observed snow amount! P 1354 line 17, this model is perhaps more process based than a statistical model, but it still lacks several processes. . . P 1355 line 17, sentence has a complicated structure. P 1356 lines 14-20. This discussion is somehow out of place. P 1357, line 1. Perhaps best, but also includes great sources of error. . . Beginning from lines 23 and 27. Sentences have complicated structure. Line 25. seem to be – are? P 1358, line 1, in order to

Table 2 “only positive values” – is this needed? In the footnote and in the table different elevation ranges are given. What can you say about locating the stations in respect to forest cover and slope angle – not only to elevation?

***Author’s response:*** *Most of the Reviewer 1’s technical corrections and suggestions for better wording are taken into account in the revised ms. The present value for the snowfall/rain threshold temperature parameter Ts could potentially be adjusted in future model calibration. However, preliminary analysis of air temperature and snow depth data from the Norwegian meteorological institute suggests that Ts lies quite close to the present default of 0.5° C. The precipitation correction parameters (fr, fs) are set to 1 in all model grid cells at the moment (i.e. no correction of input percipitation). However, in future model calibration they might be adjusted, e.g. depending on grid cell elevation, to remove the model bias seen in SWE (assumedly due to overestimated input precipitation). The two parameters in the equation for Cm presently are more or less based on expert opinion from the time of the model launching in 2004, as well as some evaluation of the model results against observations (see Engeset 2004a, b, cited in the ms.). However, these parameters may need to be adjusted in future model calibration. Cm in the current model version is a function of forest cover, latitude and time of the year. If the model would in the future be extended to simulate the fraction of snow covered area within the grid-cells, this would be a natural extra parameter to be included in the equation for Cm, reducing the grid-cell-averaged melt rates in the partly snow-covered grid-cells in the late melting season. The setting of parameters fs and Cm_max is now briefly mentioned in the revised ms. (Table 2).*

*It is correct that snow density is calculated from SWE and snow depth, However, in the compaction model both snow depth and density could be used as variables, as SWE is calculated before the compaction algorithm. Thus, overestimation of modeled snow density is a consequence of the parameterization and formulation of the compaction algorithms in the model (i.e. resulting in too much compaction).*

“ why in February?”: *this arises from the different measurement plans/schedules of the different hydropower companies.*

“you did not use original density observations?”: *strictly speaking, the hydropower companies report snow depth and SWE, although they measured snow depth and density.*

“why so?”: *this must be due to the fact that the current density algorithm overestimates compaction of especially snow with lower density. This is now briefly mentioned in the revised ms.*

“kg/l is not SI-unit.”: *True for “liter”, but “liter” is actually officially accepted to be used with the SI-system (see: http://www.bipm.org/en/si/si_brochure/chapter4/table6.html).*

“this model is perhaps more process based than a statistical model, but it still lacks several processes.”: *“more process-based” is now used in the revised ms.*

“only positive values” – is this needed?”: *“values above zero” is now used in the revised ms. in order to clarify the meaning, that the uncertain bare ground observations are not used.*

"In the footnote and in the table different elevation ranges are given.": *These elevations are not directly linked to each other, and the percentiles are rather arbitrarily selected, therefore the elevation ranges need not to match.*

"What can you say about locating the stations in respect to forest cover and slope angle – not only to elevation? " *: The met.no- and HPC-stations exist both above and below the treeline, although a majority of the met.no-stations are below the treeline and vice versa for the HPC-stations. At the moment we do not have information on the slope angle available, but in principle this would be possible to obtain from topographical maps. The point-based met.no stations are normally situated on rather flat ground, while the snow course-based HPC-stations with areal extent (normally ~1 km long transects) cover a variety of different slope angles.*

**Reviewer #2**

The research objectives of are clearly stated and the discussion and results answer those objectives. A very through description of the seNorge model and analysis of model outputs. Analysis is thorough; explains model output compared to observations over different regions and times so has valuable spatial and temporal information regarding seNorge accuracy and biases. Limitations on use and quality of the seNorge snow model results are well presented. A well written manuscript with very good quality graphics. Its a good contribution to the community.

I think that the manuscript could be accepted as is, though I do have comments that the author may want to consider a response to in the manuscript. One comment is a question to the author about the possibility of using data assimilation in the model or if data assimilation is a technique that could improve model output. A response to that question might be appropriate in the Conclusions and is up to the author's discretion.

***Author's response:*** *Good suggestion, assimilation techniques might well improve modeling of snow cover in Norway, and this is one of the future considerations in the model development work. The lack of frequent SWE and density data might, however, set some limitations on the assimilation. The number of observations might need to be increased in that case, as pointed out by Reviewer 2. Satellite images could also provide useful data for assimilation. Since this is an idea for future model development work, we choose not to discuss this theme further in the ms.*

Specific comments Section 2: Are the SWE model and Snow pack compaction models good models: are they physical correct in representing SWE and snow compaction? There needs to be some discussion of the accuracy of the model parts, in addition to the discussion and analysis of the outputs.

***Author's response:*** *All models remain as abstractions of the reality, and in my opinion, whether a model is good or not is usually strongly related to the purpose it is used for. A more complicated and physically detailed model would in the Norwegian snow mapping case likely require more simulation-based meteorological input data and more computer processing time and have more parameters that must be adjusted. Thus, the present model system seems, in my opinion, to be quite well-fitted for most of the snow mapping purposes (mainly hydrological), maybe with exception of avalanche forecasting, which* *requires some more detailed information on vertical layering of snow. In my opinion, the seNorge model system represents a rather good compromise between practical applicability, physical process representation and demand for input data. In the revised ms., the potential sources of uncertainty, reasons for the "not-so-good" model fit as well as processes to be developed further in future model versions are somewhat more discussed and specified, as also suggested by the Reviewer 1 (see first response to Reviewer 1 above).*

Pg 1355 line 25, and Conclusion: Is seNorge re-calibration the only option? Since there are limited SD, snow density and SWE observations made over all of Norway, would an increase in the number and frequency of those measurements increase model accuracy and decrease biases? Could snow observations be assimilated into the model? Probably not possible to assimilate into seNorge as is but, is assimilation a technique that could improve modeling of snow cover in Norway?

***Author's response:*** *The increase in the number of snow observations would not directly increase model accuracy and decrease biases (as long as they are not assimilated into the model to correct the snow maps). However, a better coverage of temperature and precipitation measurement stations, especially at the higher elevation sites, would contribute to more accurate interpolation of the temperature and precipitation input data, and so also probably increase the accuracy of the simulated snow maps. This is now briefly mentioned in the revised ms. See also the response to Reviewer 2 four paragraphs above.*