# Authors response to the reviewers

We express our sincere thanks to the two reviewers, whose comments testify from a very careful reading and helped us improve the quality and focus of our manuscript, as well as the scientific robustness of our results.

## 1. Response to common comments

**English language (Reviewer # 1, minor comments; Reviewer # 2, comments 3, 4, 8)**:
As a response to a common demand from both reviewers, we rewrote and cut a number of sentences while also correcting the English language with a special focus on the words 'centimetric', 'evolve', 'respectively', as well as on the use of singular and plurals. We hope that those revisions increased the clarity of the manuscript.

**Soil moisture anomaly (Reviewer # 1, minor comments; Reviewer # 2, comment 5)**:
Both reviewers pointed out that the statement "as soil moisture is only modeled in terms of anomaly" (p 2220 line 17 of the original manuscript) is inconsistent with the description of the hydrological scheme with soil freezing performed in Section 2. We agree with their comment and admit that the term "anomaly" is inappropriate to characterize the way soil moisture is represented in the model. Indeed, ORCHIDEE makes use of and diagnoses a vertically-discretized volumetric soil moisture content. The point we wanted to make by using the term "anomaly" is that the model is only able to represent with a certain degree of accuracy the *variations* of the soil moisture content, as this variable is initialized with an arbitrary, potentially inaccurate, value, and as only the soil water *fluxes* are computed by the model. We therefore can not retrieve the *absolute value* of the soil moisture content, and the modeled soil moisture content may include a constant bias.
However, this argument does not hold for the uppermost part of the soil (and the 0-20cm layer investigated for our soil moisture comparison belongs to it), which is in direct interaction and equilibrium with the atmosphere at the latitude of Valdai, where the annual evaporation approximately amounts the maximum water content of the first meter of the soil.
We therefore eliminated this argument from the manuscript.
However, we still feel that the marginal (<2.5%) difference in the 0-20cm average soil moisture content between the linear and thermodynamical simulations at Valdai can not be used to ascertain that one parameterization is better than the other, as this model behaviour may be due to site specific conditions. The reproducibility of this behaviour and of a better agreement with data for the thermodynamical parameterization, should be verified at other cold-region sites prior to establishing a firm conclusion. We used this occasion to insist on the limits of our single-site approach for hydrological validation purposes (revised manuscript, section 4.1, §5).

## 2. Response to specific comments by Reviewer #1

**Major comment 1: motivation and length of the comparison to the Stefan solution.**
The reviewer pointed out that the motivation of Section 3.1 dedicated to the comparison of model outputs to the analytical solution by Stefan (1890), did not appear clearly in our manuscript. We therefore clarified our goals and edited a new section 3.1, entitled "Methods and data". This section synthesizes the motivations of our comparisons of model output to "idealized" datasets and introduces to the characteristics of the selected datasets.
We enclosed this new section 3.1 in our response to the reviewers:

*"**3.1. Methods and data***

*This section aims at evaluating the ability of the new soil freezing scheme to represent the thermal and hydrological processes involved in soil freezing and thawing, and at determining the range of validity of key numerical parameters. For this purpose, model outputs are compared to idealized data. By "idealized data" we mean data where the unknowns usually restricting the power of model validation (uncertainties in the atmospheric forcing, uncertainties in the soil and vegetation parameters, errors or error compensations due to processes not represented by the model) are minimized. In those conditions, the numerical performance of the algorithm and the suitability of the numerical choices (spatio-temporal discretization, freezing window) can be examined. The mere ability of a scheme to represent a desired process with a desired degree of accuracy is not straightforward as the performance of numerical algorithms are known to be likely sensitive to numerical parameters (e.g. De Rosnay et al., 2000). A scheme validated in idealized conditions does not necessarily perform well in real climatological conditions. However, establishing the validity and conditions of validity of a numerical scheme is a preliminary step in the validation process.*
*Regarding soil thermal processes, the analytical solution of the freezing front progression by Stefan (1890) allows to test the model in idealized conditions: the thermal parameters and boundary conditions used by the model can be set identical to their counterpart in the analytical solution. The accuracy of the scheme can then be investigated in unbiased conditions, and its sensitivity to numerical choices can be explored.*
*To our knowledge, there is no simple analytical solution to the problem of liquid water movements in frozen or partially frozen conditions as described by equation (1). We therefore relied on a laboratory experiment to benefit from an explicit setup and well measured soil parameters which could be used in our model. The Mizoguchi (1990) experiment described in section 3.3 provides such conditions. This experiment is used as a benchmark for the hydrological parameterizations.*
*This methodology ensures the stepwise validation of the whole soil-freezing scheme: it first focuses on thermal processes alone (see 3.2), then both thermal and hydrological processes are considered, which is necessary due to their intrication. The one-dimensional simulations involved span very limited time ranges (around 50h) and are of low numerical cost; they are thus particularly suited for sensitivity tests."*
In short, Section 3 essentially aims at a numerical validation of the scheme and focuses on sensitivity tests performed on numerical parameters (spatio-temporal discretization as well as "freezing window"). These tests are designed to establish the range of validity of our scheme. We completely agree with the reviewer that the validation of a new scheme against

idealized datasets does not warrant its performance in real climatological conditions. However, we considered this step of preliminary numerical validation as necessary in the process of model development, and also worth mentioning in a publication dedicated to the description and validation of a new scheme.

We also agree with the reviewer that the former Section 3.1 comprised some overlong passages and could readily be shortened. In particular, the theoretical considerations on the relationship between the model speed and the phenomenon speed (p 2213 line 14 to p 2214 line 2) as well as the theoretical constrains on the spatio-temporal discretization under a given freezing window (p 2214 line 3 to p 2214 line 16) appeared to us of lower relevance in a paper dedicated to model description and validation. Accordingly, we shortened the manuscript, eliminated the duplication of information with the new section 3.1, and put our efforts to highlight the main conclusions of the former Section 3.1: (i) The fact that a finer-than-default vertical resolution (up to 5mm) does not degrade the performance of the soil freezing scheme. We strengthened this conclusion by mentioning more simulation results using finer vertical resolutions up to a regular vertical spacing of 5 mm in the uppermost meter of the soil. (ii) The fact that a 2°C freezing window is both physically realistic and compatible with the default spatio-temporal resolution of the model. We also improved the clarity of Figure 3 by suppressing the dotted lines relative of the improved resolution. All the figures which were subject to revisions are displayed at the end of this response to the reviewers.

We hope that this thorough revision improved the focus of the manuscript while preserving the robustness of this section highlighted by the second reviewer.

**Major comment 2: improper use of a snow-depth dataset.**

The reviewer pointed out that the snow depth dataset by Foster and Davy (1988) is a climatological dataset and does not refer to a specific year, contrary to what we assumed in our manuscript.

We recognize that we failed to notice this important aspect of the Foster and Davy (1988) dataset and thank the reviewer for pointing out this mistake. In our original manuscript, this snow depth dataset is used at two occasions in Section 4.2.1 to point out the origin of a major thermal bias in ORCHIDEE, namely the improper representation of the snow cover.

The dataset is first used in Figure 8 to discriminate the points with proper, underestimated or overestimated snow in our comparison of the modeled vs observed soil temperatures at HRST stations for the year 1987. The comparison of the modeled snow depth to observed (composite) records is useful because it helps quantifying the reduction in model bias at the points where the snow is properly simulated. As comparing the climatological snow depth data to the simulated snow depth for the year 1987 can not prove the inaccuracy of our modeled snow depth for this specific year due to interannual variability, we performed the following changes in the revised version of the manuscript.

- the monthly averaged model snow depth for the period 1984-1994 is compared to the climatological dataset to diagnose whether snow is overestimated, underestimated or correctly modeled at the station site. This comparison leads to the symbols used in Figure 8.
- Furthermore, we now compare the 1984-1994 mean model soil temperatures at the HRST stations to the 1984-1994 mean observed soil temperature. This ensures that the accurate or inaccurate representation of snow in the model is assessed over the

same period than the model-to-data comparison is carried out, hence can explain part of the model bias.

Carrying out this comparison required time due to the treatment of new data but we feel that the comparison of decadal means increases the significance of our results. This revision does not change our conclusion on the role of snow in the systematic model bias.

We also used the Foster and Davy (1988) dataset for a forced-snow experiment carried out at Iakutsk (Section 4.2.1, Figure 9), which pointed out a major reduction in model bias when the model was thought to use a proper snow. However, this result was inaccurate since we forced the model for the year 1987 with climatological, not year-specific, snow depth data.
To establish a robust conclusion on this point, we decided to compare the results of a 10-yr simulation with the default modeled snow depth, to a 10-yr simulation where snow depth was forced by the climatological data. Those simulations were carried out at Iakutsk over the decade 1984-1994. The Figure 9 of the revised manuscript shows the results of this comparison, which are very similar to those exhibited in the original manuscript. We therefore only changed the description of the numerical experiment in the revised manuscript, and left the conclusions unchanged.

**Further minor comments**

**On the numerical nodes in Table 3.**

As pointed out by the reviewer, the description of the vertical discretizations of the thermal and hydrological schemes performed in Section 2.2 was not consistent with the values given in Table 3. The confusion arises because Section 2.2 describes the thicknesses of the layers, whereas Table 3 gives the values of the numerical nodes, *which are not the centers of the layers*. We definitely should have mentioned this point to ensure the clarity for the reader. Furthermore, our description of the discretization of the hydrological scheme was inaccurate: in the hydrological scheme, the quantity following a geometric sequence of ratio 2 is *the spatial step between the numerical nodes*, and not the thickness of the layers as stated in our original manuscript. This inaccuracy was corrected in the revised version of the manuscript, and a reference available in English language is added (de Rosnay et al., 2000).
As the formula used to retrieve the numerical nodes of the thermal scheme are not trivial and do not add much to the description of the scheme, we decided to simplify things and mention in Table 3 the depths of the boundaries of the layers for this scheme. Similarly, the calculation of the layer thicknesses for the hydrological scheme is not straightforward and computation of equation (1) is made at the numerical nodes anyway, so we only listed the depths of the numerical nodes in Table 3. The drawback of this choice is an inhomogeneous table mixing depths of the boundaries of the layers on the left side, and numerical nodes on the right side. However, it corresponds to the values effectively used for modelling purposes and mentioned in the cited model documentation. We hope that this choice will satisfy the reviewers. We enclosed the Table 3 designed for the revised manuscript at the end of this response to the reviewers.

## 3. Response to specific comments by Reviewer #2

**General comments**
Emphasize was judicially set on the need to insist on the limits of a single-site approach, especially for the purpose of hydrological validation, and on the reduced quality of the edition of Section 4. We thank the reviewer for pointing out these aspects and tried to improve our manuscript following those guidelines, as will further appear in the response to the next comments.

**Comment 1.** We agree that several land-surface models are now coupled to soil carbon models within coupled climate carbon cycle models. The land surface model ORCHIDEE stands somehow out among all land surface models because it has been since the origin built around soil carbon (ORCHIDEE stands for Organizing Carbon and Hydrology In Dynamics EcosystEms). When other models can be "coupled" to a soil carbon cycle, it is more exact to say that the soil carbon cycle can be "activated" or "deactivated" in ORCHIDEE. This explains the emphasize we wanted to put on the direct added value of soil freezing for soil carbon applications in ORCHIDEE. However, our statement was inaccurate. Therefore, we decided to simply put a general emphasize on the importance of the thermal, hydrological and biogeochemical implications of soil freezing (with discretized hydrology) for carbon cycle modelling (Introduction, end of § 5).

**Comment 2**. The origin of the latent heat exchanges within the soil (soil water freezing and melting) was detailed for more clarity.

**Comment 6.** We completely agree with the reviewer that strong conclusions can not be drawn from model output-to-data comparison at one single site, especially regarding hydrological processes and in link with the extreme spatial variability of soil hydraulic properties. Furthermore, the Valdai site may be to some extent representative of regions undergoing seasonal freezing. However, it is not located in a permafrost region and therefore does not allow a validation of permafrost-related processes. This further reduces the extent of our validation. Following the suggestion of the reviewer, we insisted on this aspect at the end of Section 4.1 and mentioned two cold region and permafrost sites where further validations are planned.

**Comment 7.** We thank the reviewer for correcting our misuse of the term 'active layer' and used 'active layer thickness' or 'maximum active layer thickness' instead when appropriate.

**Comments 9, 10**. We agree with the reviewer's comment and mentioned in the revised manuscript that the introduction of organic matter would likely increase the model cold bias in summer. We also added a comment on the likely effects of the representation of organic matter on the modeled active layer, concluding that the physical realism of our model is still poor to really take advantage of a model-to-data comparison. The suggested references were cited.

**Comments 11, 12, 13.** We accepted and applied the reviewer's suggestions and corrections.

**Comment 14.** As we agree with the reviewer that Figure 11 is dense and hard to read, but still are reluctant to give up the message conveyed by the dotted line (drainage induced vs runoff induced discharge), we decided to have one plot with all lines (Lena, thermodynamical parameterization) and simplified the plots for the other rivers.

## References

De Rosnay, P., Bruen, M., Polcher, J. : Sensitivity of surface fluxes to the number of layers in the soil models used in GCMs, Geophys. Res. Lett., 27, 3329-3332, 2000.

Hourdin, F. : Study and numerical simulation of the general circulation of planetary atmospheres, Ph.D. thesis, Université de Paris VII, 1992.

Foster, D. and Davy, R. : Global snow depth climatology., Tech. rep., USAFETAC/TN- 88/006, Scott Air Force Base, Illinois, 1988.

Stefan, J. : Über die Theorie der Eisbildung, Monatshefte für Mathematik, 1, 1–6, 1890.

# Revised Table and Figures

| **Layer n°** | **THERMAL MODULE**<br>***Depths of the layers boundaries (m)*** | | **HYDROLOGICAL MODULE**<br>***Depths of the numerical nodes (m)*** |
|---|---|---|---|
| | **Default resolution** | **Extended depth** | |
| 1 | 0.043 | 0.043 | 0.0 |
| 2 | 0.129 | 0.129 | 0.00195 |
| 3 | 0.301 | 0.301 | 0.00586 |
| 4 | 0.646 | 0.646 | 0.0137 |
| 5 | 1.34 | 1.34 | 0.0293 |
| 6 | 2.72 | 2.72 | 0.0606 |
| 7 | 5.47 | 5.47 | 0.123 |
| 8 | | 10.99 | 0.248 |
| 9 | | 22.02 | 0.498 |
| 10 | | 44.09 | 0.999 |
| 11 | | 88.23 | 2 |

**Table 3.** Vertical resolutions of the thermal and hydrological modules in the default configurations, and in the extended-depth configuration (thermal module only).

**Fig. 2** Freezing front progression as calculated by the Stefan solution (STEFAN) and simulated by three ORCHIDEE simulations: without soil-freezing (NOFREEZE); with the soil-freezing thermal algorithm at the default resolution (FREEZE, default res), and with the soil-freezing thermal algorithm at an improved resolution (FREEZE, improved res). The horizontal dashed lines mark the positions of the vertical nodes in the default resolution.

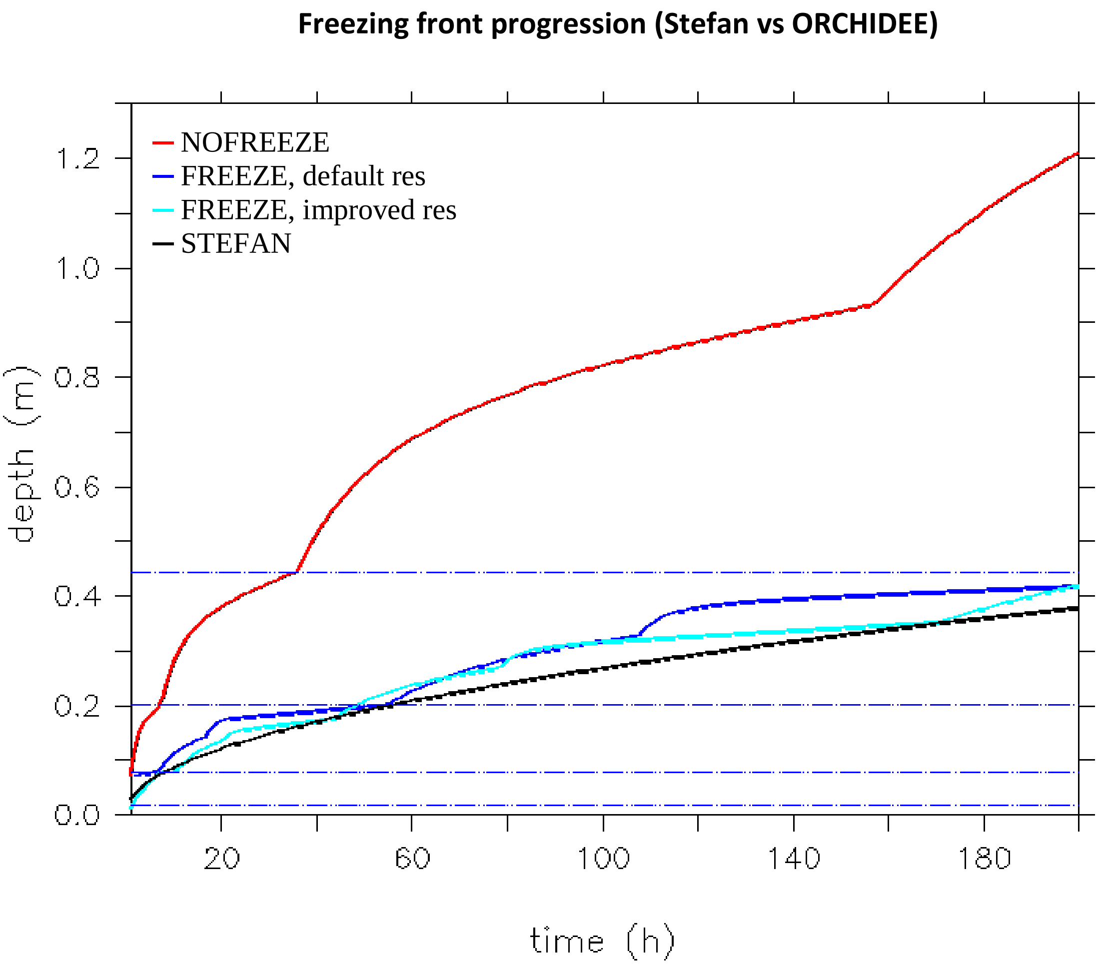

**Fig. 7** 20cm soil temperature difference between the model with and without the new soil freezing scheme. Top: seasonal averages over the (1984-2000) period. For each season, the averaged months are mentioned by their initial in brackets. Bottom: annual average over the (1984-2000) period.

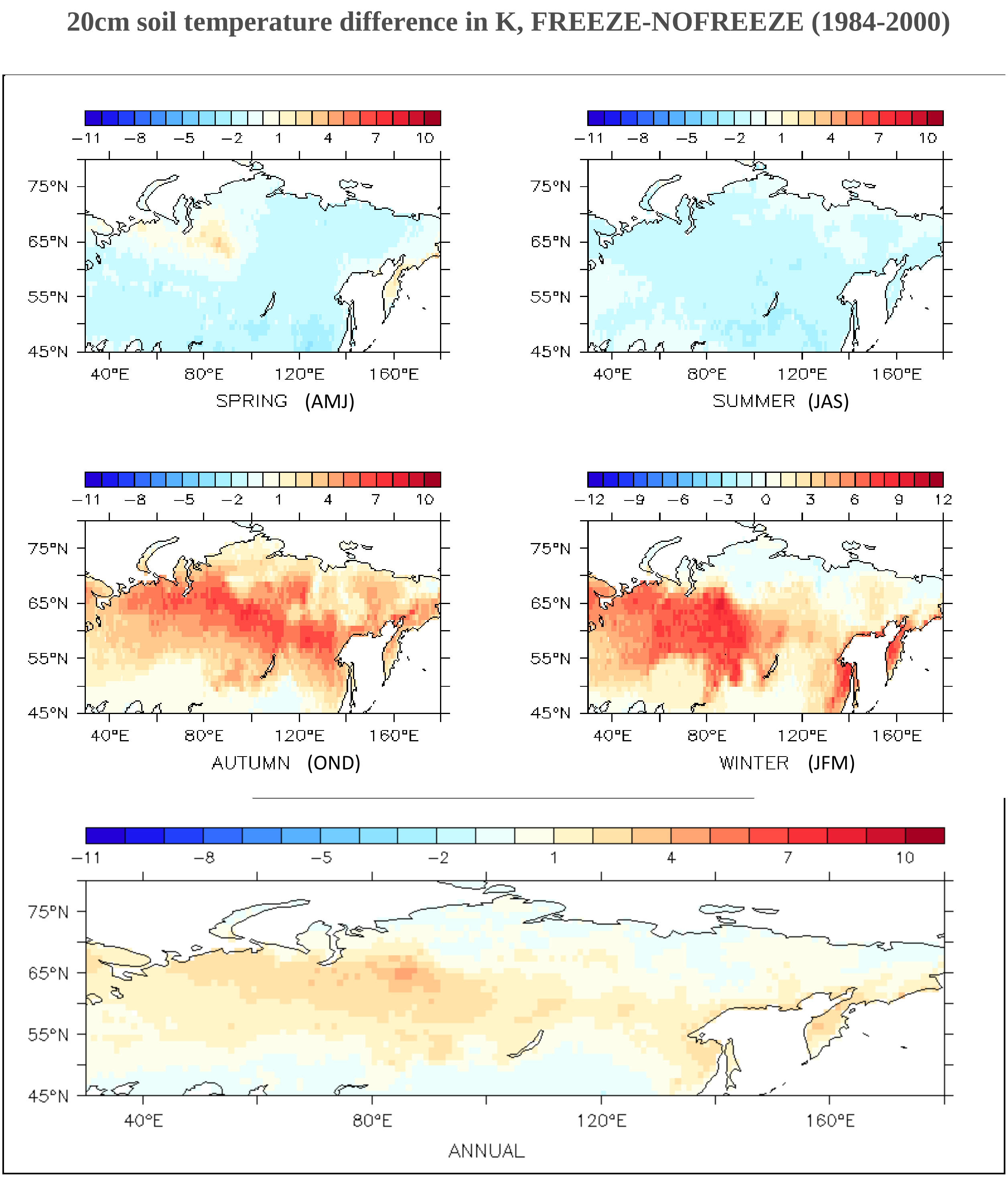
20cm soil temperature difference in K, FREEZE-NOFREEZE (1984-2000)

**Fig. 8** Top: HRST stations locations. Bottom: comparison between observed (x-axis) and modeled (y-axis) soil temperatures at depths from 0 to 20cm at the HRST stations, averaged over the decade 1984-1994. Colors refer to the model with and without the soil new soil freezing scheme; symbols discriminate between the sites where the snow depth is either properly represented by the model within a +/- 20 cm range (correct snow), or underestimated by the model (snow underestimation) or overestimated by the model (snow overestimation) when compared to the climatological dataset by Foster and Davy (1988).

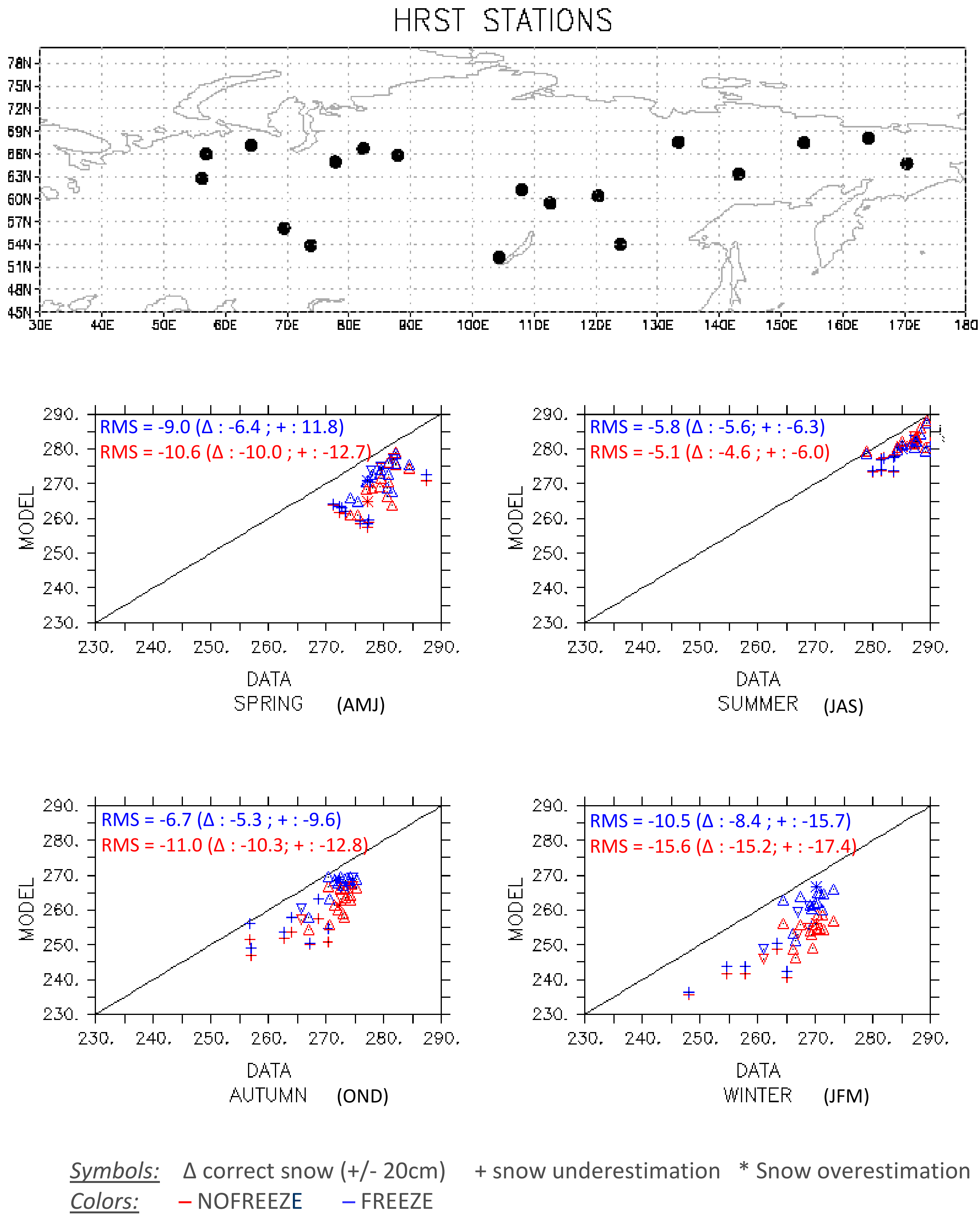

**Fig. 9** Comparison between modeled and observed soil temperatures (in K) at different depths (shallow, deep, very deep) at Iakustk in an experiment with the default model snow depth (left) and in a forced-snow experiment (right), where the modeled snow is artificially forced by climatological snow depths.

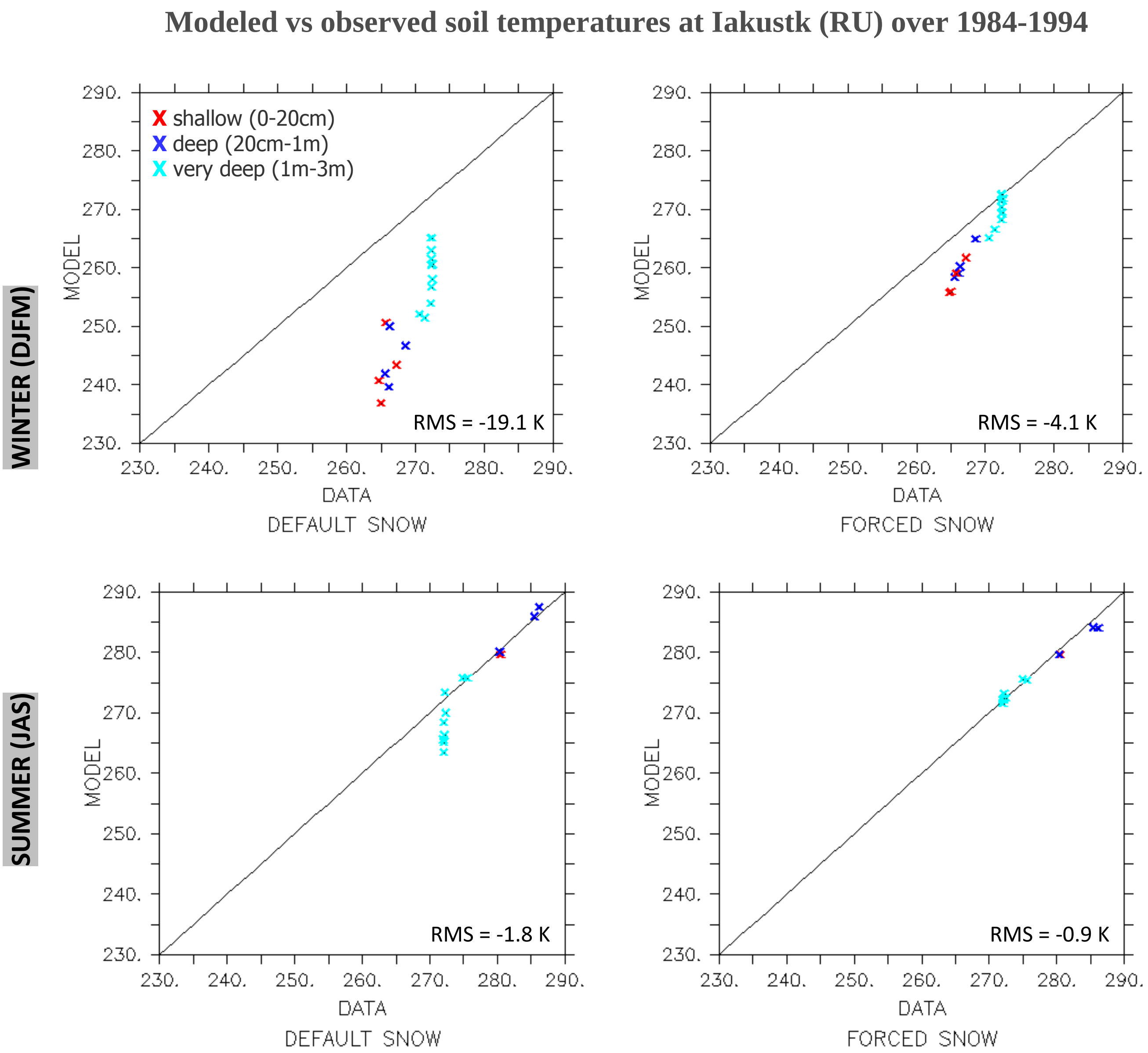

**Fig. 11** Mean annual hydrological dynamics of the three main Siberian basins, as simulated with (FREEZE) and without (NOFREEZE) the new soil freezing scheme for the decade 1984-1994, and river discharges from the R-ArcticNET database (DATA). In the upper part the soil freezing scheme uses the thermodynamical parameterization; in the lower part the linear parameterization is used. Plain curves represent the hydrographs at the mouth of the rivers, thin dotted lines the drainage at the bottom of the soil column, and large dotted line the surface runoff. "Err" refers to the mean model error in the cumulated annual discharge over the basins.