Geosci. Model Dev. Discuss., 6, C3097–C3109, 2014
www.geosci-model-dev-discuss.net/6/C3097/2014/

Geoscientific Model Development Discussions

Interactive comment on "GEOtop 2.0: simulating the combined energy and water balance at and below the land surface accounting for soil freezing, snow cover and terrain effects" *by* S. Endrizzi et al.

S. Endrizzi et al.

stefano.end@gmail.com

Received and published: 18 July 2014

The comments of the reviewers are in bold, the answers of the authors in italic. Where possible, we followed the suggestion of the reviewer and fulfilled their requests.

This paper presents a novel modeling approach to simulate the surface energy and water budget in mountain terrain, solving in a coupled way water and energy fluxes, considering snow and freezing soil dynamics in a numerically consistent way. The presented model could lead to major advances in cold processes sci-

ence and engineering applications, and I recommend its publication in GMS after minor revision. The paper presents a new version of the existing GEOtop model with significant new improvements. The GEOtop model has been already widely applied in a variety of contexts, ranging from catchment scale hydrological applications, hillslope hydrology, runoff prediction, surface energy fluxes and vegetation dynamics, snow modeling, comparison with remote sensing products. The application presented in this paper is limited to snow and permafrost modelling over bare soil. However, the GEOtop 2.0 simulator, presented for the first time in this paper, could also be applied to the above-mentioned research topics. This enhances the impact of the work presented in this paper. This is also a point that could be better highlighted in the paper introduction.

We thank the reviewer for the appreciation of our manuscript and the very detailed review. We want to precise that the paper is not limiting to snow and permafrost modeling, but describes the most important components of GEOtop. These are given by the joint solution of water flow and heat equations in the soil, which are two highly non-linear equations. The soil freezing-thawing processes contribute to significantly increase this non-linearity, especially in proximity to the freezing point. The energy and water balance is also solved for the snow cover. Therefore, with soil freezing and snow, the most complete case is considered. Vegetation is treated in GEOtop in a simplified manner in order to only take into account its effects on energy and water balance at the surface, namely the upper boundary condition. However, vegetation is not described here because it is not considered essential for the description of the main structure of the model and it was extensively tackled in another paper (Endrizzi and Marsh, 2010). Other applications with GEOtop have been performed in realistic configurations, and most of these are cited in the paper.

Regarding the paper content, I believe that the presentation of the model is accurate and the validation exercises are sufficient and present quite good results. However, I have some minor concerns mostly regarding: The organization of the

Introduction. Some parts could be rearranged in order to improve clarity and logical flow. See specific comments with some suggestions.

We answer the specific comments.

The presentation of what is different in this GEOtop version with respect to the previous version On the one hand, the major advancements in this version are substantial and could be better underlined in this paper (see previous comment). On the other hand, the contributions of the developers of the previous versions of the code could be acknowledged more in detail.

The differences with respect to the previous versions are described in the introduction from page 6282 line 23 to page 6283 line 9. In the Acknowledgments part the developers are thanked, mentioning their key contributions in past versions.

The presentation of the results and validation exercise This part is quite compact and some good results are moved to the supplementary material. The presentation lacks of many details on test sites characteristics and input data that can be useful to better understand the results. I understand the authors' choice, given the length of the paper and the space needed for the model description. I understand also how, when a new model is published, we reviewers want a detailed model description, an extensive validation and a short article, which are needs almost impossible to fulfil altogether. So I leave to the authors to evaluate what is feasible with a reasonable amount of work. I propose two options to improve this part: 1. move the very technical paragraph 5.6 to the Appendix and expand the paragraph 6 including some figures from the supplementary material. Please see the suggestions the specific comments section. 2. Split the paper in two, if the Journals Editor allows it. GEOtop 2.0 (I) model description; GEOtop 2.0 (II) model test and validation. This second option would certainly require more work, but the valuable results shown in the supplementary material could deserve a second paper.

We moved paragraph 5.6 to the Appendix, and we expand paragraph 6 by including the material contained in the supplementary material (please see also the detailed comments). We saw it was not feasible for us to split the paper in two. Further testing will accompany future papers by the authors and, we hope, by the community using GEOtop.

Specific Comments

... **Maybe also a template for the simulations presented here could be added. The model manual is not fully updated to the presented code version, but I trust the Authors it will be available soon.**

Most of the meteorological data are not of public domain, so we cannot make them available. However, where possible, we added simulation templates. We are also currently working to upgrade the manual. This material can also be found at: http://abouthydrology.blogspot.it/2013/10/geotop-20.html, made available from the Authors.

This abstract is quite "minimalistic". You may specify that the model allows for a complete treating of complex terrain. Also the potential of the model in terms of snow modeling and hillslope hydrology could be highlighted.

The reference to complex terrain is implicitly where we say that GEOtop considers "the radiative fluxes" because without treating the complex terrain we cannot calculate the radiative fluxes. The reference the potential of the model in terms of snow modeling and hillslope hydrology is also implicit in the abstract when we say that "describes the three-dimensional subsurface water flow" and "describes the temporal evolution of water and energy budgets in the snow cover". We have reformulated some sections of the abstract to make this more obvious.

Introduction is complete in terms of contents, but I've found sometimes hard to follow the logical flow. Below are some suggestions on how to improve it. P6281

Line 4-12 where GEOtop is introduces. It seems to me this block alters the logic flow. It would better place it at page 8282 after line 17

We rearranged the blocks, and first discuss other models and then GEOtop.

P 6280 Line 21- Please add a reference (may be some work of K. Beven? Or of R.A Freeze, 1969? Freeze, R. A., Harlan, R. L. (1969). Blueprint for a physically-based, digitally simulated hydrologic response model. J. Hydrol., 9, 237-258.)

We added reference to Freeze and Harlan.

P 6281 Line 7 "GEOtop covers the full spectrum of hydrological fluxes". This seems, on my advice, a little reductive. GEOtop is much more than this and has a much larger application spectrum. Other processes can be modeled. In fact, a unique feature of GEOtop is the fact that the new developments presented in this paper could be applied also in other contexts. This allows GEOtop to model the interaction with other hydrological, ecological and geomorphological processes in an interdisciplinary research framework (runoff production, evapotranspiration, vegetation, soil moisture dynamics, hillslope stability). Please incorporate this kind of considerations in the text. Some more references on the model could be added. See also the considerations at the beginning of the general comment section.

We added: "GEOtop covers the full spectrum of hydrological fluxes, represents the energy balance in the complex terrain, and represents snow and vegetation. Therefore, it allows modelling the interactions between several hydrological, cryospheric, ecological and geomorphological processes in an interdisciplinary research framework. For example, it has been applied to model geomorphological processes like landslide (Simoni et al., 2008), ecohydrological processes (Bertoldi et al., 2010; Della Chiesa et al., 2014), evapotranspiration (Kunstmann et al., 2013), and runoff production (Endrizzi et al., 2011). However, this paper mainly focuses on the aspects related to the cryosphere."

P 6281 Line 12-19 the block where permafrost models are reviewed and P 6281-2 Line 20-7 the block where distributed hydrological models are reviewed. Paper writing praxis recommends moving from general to specific, so I suggest to shift the two blocks. Also, for symmetry reason, a table for hydrological models similar to Table 1 can make this paragraph smoother.

We do not agree that this would improve the argumentation in our text. Since we start mentioning freezing soil and snow and their mathematical representation, we discuss first models that deal with cold region processes. Then we move discussing other models dealing more extensively with hydrology. A table for these types of model is more difficult to write due to the larger variability of modeled processes with respect to permafrost models, and also it does not significantly add value to the paper.

P 6283 Line 14-17 This last sentence is almost "philosophical" and quite difficult to understand. Please extend it and explain better. I've understood the meaning only after I came trough Figure 4. Mention here also Appendix A. It is not mentioned elsewhere in the text.

The order of the appendices has changed, also to accommodate the requests of the other reviewer. The sentences mentioned are taken from a short summary of the sections. We prefer not to mention figures in the summary.

P 6284 Line 5 Here I would put in the main body text the first two equations of Appendix B B1 and B2. It could be clearer introducing the main two heat and mass equation in the main text. Then leave all the other calculations in the Appendix.

Equations B1 and B2 are taken from Dall'Amico et al. (2011) and they are nothing more than Equation (1) and (7) written for clarity in a slightly different form. We show how the equations are writing in this form in Appendix B. We do not think it is worth to include equations B1 and B2 in the main text, because they are not more explicative than Equations (1) and (7). We have instead added in the text a few sentences explaining *better the meaning of Equations (1) and (7).*

P 6284 Line 21 What do you mean with gauge pressure?

Gauge pressure is the water pressure relative to the standard atmospheric pressure.

P 6285 Line 12 "freezing soil characteristic curve" Which equation is? What about move the paragraph 2.1.1 here and make a separate paragraph for the thermal conductivity parameterization?

In the text we say that a freezing soil characteristic curve is fixed relations between unfrozen water content and temperature. In this paper a definition of the freezing soil characteristic curve as a functional dependence (water content only function of temperature) is sufficient in order to properly explain the heat equation and its solving method. An exact mathematical relation is not needed, and it is referred to Dall'Amico et al. (2011) for details. We added further details regarding the freezing soil characteristic curve in paragraph 2.1.2. We made a new paragraph in the text for the thermal conductivity.

P 6286 Lines 7-13 This could be moved before, immediately after mention Eq. 2.

We followed the instructions of the reviewer.

Eq. (9) Psi is negative in unsaturated soil?

Psi is defined as a (gauge) pressure head, therefore it is negative when soil is unsaturated.

Par 2.3 Water flow equation. Is soil storativity considered?

Yes, soil storativity is considered, we added a sentence in the text explaining this.

Eq. (13) I do not understand well this equation. I went trough the Gottardi and Venturelli paper and I did not found this equation. I assume some new derivation it has been made. Could you add some further details on the physical meaning

of the different terms of this equation?

Answer: We added further explanations in the text. We did not use exactly the same equations used in the Gottardi and Venturelli, but we used their general principle, which is extending the validation of the Darcy law to the overland flow, although it would not be valid since the motion is turbulent. This was accomplished modifying the hydraulic conductivity. If in a turbulent flow $F = -k(\nabla p)^{0.5}$*, where* F *is the flow per unit surface [m/s],* k *the hydraulic conductivity [m/s] and* p *the pressure head [m], we can write, which* $F = -\frac{k}{(\nabla p)^{0.5}}\nabla p = -k^{'}\nabla p$ *, which virtually respects the Darcy law.* $k^{'}$ *is modified conductivity.*

P 6292 Lines 9 I would add for more clarity: "Then, in Section 4 the case with complex terrain is presented."

Done.

Par 3.1 Shortwave radiation. Before it can be mentioned that in the model, depending on the input data available, radiation components can be either assigned directly in input or calculated by the model.

Done

Eq. (22-24) mo and w are external input parameters or are parameterized by the model? And how?

They are input variables and assigned as parameters.

P 6293 Lines 25; P 6295 Lines 20; P 6296 Lines 13 What happens when vegetation is present? Such parameterizations will change. It might be worth to mention here that GEOtop deals also with the case of soil covered by vegetation, but that the topic has been presented elsewhere (Endrizzi and Marsh, 2010).

We cited Endrizzi and Marsh (2010), there we show how radiation and turbulent fluxes are modified when vegetation is present. We did not want to include vegetation in this *paper.*

P 6296 Lines 20; Do you mean the concept of displacement height? (Stull, 1988; Garrat, 1992)

No, we did not consider the displacement height here, since it was not necessary.

P 6296 Lines 12-13; Please reformulate. This ratio is not clear.

α_γ *and* β_γ *are parameters, there is nothing to define. The question is not clear.*

P 6299 Lines 25; Appendix D. It should C. The appendix C is not yet mentioned.

We corrected it in the text and changed the Appendices.

P 6300 Lines 16; Reformulate as: The effects are also considered in a simplified way in the model.

Done.

P 6304 Lines 3; I do not understand . if the compaction rate increases with temperature, how can double with a very cold temperature of 17?

The compaction rate decreases with snow temperature, this is a mistake in the text. The word "increases" was replaced by "decreases".

P 6304 Lines 15-20 and 20-25; You discuss here some limitations, but it is not clear at the end if the model deals or not with such issues.

Yes, the model deals with these issues. This was better explained in the text.

Par 5.6 Discretization. This part is interesting and innovative. The approach with mobile layers seems smart, but it is also quite technical. To shorten the paper, an option could be to move this in Appendix.

We moved this part to Appendix D3.

P 6308 Lines 4-10; I suggest giving this information before at p 6307 line 20. A

scheme with the snow layer discretization could clarify the approach.

We followed the reviewer suggestion, but only for lines 4-5. In our opinion, lines 6-10 cannot be written before.

Part 6 Testing GEOtop. This part is also interesting and deserves to be expanded. I suggest to: Divide the text in separate paragraph for the different text performed; Include more information on the input data, study area and model setup for the Col de la Porte case study, eventually expanding Appendix C or including the appendix in the body text; To put in the paper a Figure with the modeled and observed ensemble averages of the results of Col de la Porte; A little more information on what is Crocus; Add a scheme or a Figure with and some information on the input data, study area and a model setup for the Jungfraujoch steep bedrock case study, eventually expanding Appendix C or including the appendix in the body text

The test cases previously in Appendix C were moved to the main text, in particular in Section 6, where a paragraph was assigned to each test case. Crocus is a snow model, which was tested in Vionnet et al. (2012), but it is not relevant in this section. In this work we used the same data used from Crocus to show that GEOtop snow results are plausible, but it is beyond the purposes of this paper to perform an extensive evaluation (or a "validation", see comments to Reviewer 1) of the model, in particular the snow component. For this reason we added a few more comments and figures, but we did not significantly extend this section.

Par 7 Simulation experiment. Could be possible to add in the supplementary material also the model configuration files of such a numerical experiments?

This has been done.

P 6311 Lines 5; Which is the aspect of this synthetic catchment? S-N?

It was now shown in Fig. 4.

P 6311 Lines 15; May be a table with the 6 simulation settings could be clearer.

In order not to increase the length of the paper, we just explain in the text how the six simulations were performed. In addition, in our opinion a table would not significantly add more information.

P 6311 Lines 18; Meteorological forcing is taken at 1595 m a.s.l., but the synthetic catchment is at 3000 m a.s.l. How data have been adjusted for elevation? With time-constant lapse rates?

The standard lapse rate of 0.0065 K/m was used. This was added in the text.

P 6312 Comments on Figure 3; This part is quite confusing for me. To improve it I suggest split the discussion focusing on two points. 1) The effect of model configuration (10 vs. 30); 2) The effect of slope (steeper vs. flatter hillslopes). It would be also interesting if the Authors were able to speculate if the observed differences could be related to some physical properties. Figure 3 might become clearer if you join the symbols with lines or separate in different subplots the effect of model configuration from the effect of slope.

The purpose of this paragraph is to show that different topographies and different hypotheses on soil water balance have significant effects on the results. It is beyond the purposes of this paper to investigate the reasons why these differences occur. That is the type of research we want to enable by publishing GEOtop. Doing it properly would be complex and probably deserve separate publications. Therefore, we did not split the discussion focusing on the differences produced by topographies and hypotheses on soil water balance. We prefer not to join the symbols with lines, as this would not imply valid values between the points calculated.

P 6313 Comments on Figure 4; Nice analysis! When you come to this Figure you understand the cryptic and almost philosophical last sentence of the introduction. I suggest only to put in the subplot labels of the simulation configuration,

otherwise it is difficult to understand this from the caption (i.e 1) AL 5deg 0-0; 2) AL 10deg 0-0; .)

This was considered while writing the first draft, but we did not choose this option, because the maps are very small and writing the caption directly on them makes the figure too heavy and, at the end, unclear.

P 6313 Comments on Figure 5; Figure 5 is nice and full on information, a good scientific artwork. The discussion is focused on frozen soil, but it could be possible to use this Figure also to demonstrate model's capabilities in simulating snow pack evolution. In fact, the Figure nicely shows snowpack time evolution and its ice and water content partitioning, according to snowpack transformation. Moreover, there are two interesting features that it would be good to discuss with references on the existing literature on frozen soil. The presence of an intermediate "dry layer". It has been observed in nature? The fact that only at the end summer there is a single moment in which there is a water transfer from the surface layer to the deeper layers. I guess this is due to the nonlinear behavior of soil hydraulic conductivity with respect to temperature and water content, which is a distinctive feature of the GEOtop model. The fact that deep water table recharge is a quite nonlinear process in seasonally frozen soils could have several relevant implications. Please discuss this with references to literature.

The question posed by the reviewer is interesting. However, these considerations are valid just for the application shown in this paper, in which the boundary conditions, soil variability and topography were greatly simplified with respect to reality. We are not sure that we can extrapolate the model results to other, more realistic configurations, and, at the moment, we do not want make statements about general behaviors observed in nature, being our purpose in this paper to show that the model works, and produces plausible results, and not discussing at length the physical processes we modeled, which remain, certainly, the scope for future papers.

Conclusion. Please add a final consideration that, while here only applications for cold region are presented, the model could be applied for a much wider range of environments and scientific issues.

Done. We present in the paper the cold region case since it is the most general one, which involve the largest number of processes

Appendix B. The equations are very clear. Please add some more comments on their meaning and better highlight what is new and what is deriving from existing literature.

In this appendix we repeat calculations that were already presented in Dall'Amico et al. (2011), where the meaning of the equations is extensively discussed with reference to the literature. Therefore we remind to this paper for details.

Appendix C. As already told, this part could be expanded. At least separate the different case studies in different paragraphs.

This was done.

Appendix D P 6320 Lines 9; Nothing is told on precipitation input, which data are used and how they are spatially distributed. Please add this.

All the meteorological variables, including precipitation, are spatially distributed with the with the geostatistical method of Barnes (1964). This was made clearer in the text.

Interactive comment on Geosci. Model Dev. Discuss., 6, 6279, 2013.