RESPONSES TO REVIEWER 1

May 3, 2017

Introduction
Response to Reviewer 1

Already the introduction of the submitted paper should mention the availability of such important sources and clarify what exactly the goals and added values could be of a special focus on Europe. What are the "issues" mentioned in the title: observations, process understanding, modeling, scenario development, impact assessment or communication with the public/policymakers, etc.? To what do the terms "past", "current" and "future" relate: To the historical development of observational strategies, to major steps in scientific progress, to emerging research fields, to changes in nature, to time scales of years, decades, centuries, etc.? What was the concept for the reflection and synthesis procedure beyond compiling and enumerating many publications (especially of seven key authors with their names in up to 25 references each)? Was the intention to break the results down into key messages or should this be left open on purpose? What are the logics behind the structure of the presentation?

Response: The lead authors of the manuscript will attend to major overhauls to the introduction and conclusions of this Review Paper once the chapter revisions / responses to reviews have been accepted by the reviewers. Indeed, if further reviews involving additional or modified texts are requested of the author team, these changes will obviously have an additional impact on the way the introductory and concluding chapters are redrafted, and it is probably premature to attend to these at this stage of the review process. There are a number of important and interesting points raised in these comments that will be attended to when preparing the final draft of the Review Paper.

Section 2a
Response to Reviewer 1

The subsections of chapter 2 on the cryosphere components should all follow the same scheme for better comparison; section 2.1 is a good example with a brief general introduction followed by a summary of observed changes and then a discussion of likely future changes.

> Response: We agree that it is very benefitial to keep the sections comparable by using a similar structure as in section 2.1. The intro of section 2.4 has accordingly been further improved/clarified by a slight re-arrangement of the text in that sense.

Snow and ice research in the Scandinavian mountains seems to be rather weakly represented and options offered by the explosive development of remote sensing capacities and new surveying technologies could be a stronger focus.

> Response: We added a few more references from the Scandinavian mountains and rephrased the corresponding and the final concluding paragraph. We checked for current literature describing changes based on remote sensing. However, we could not find any since mountains regions were often explicitly excluded or the available time series of newer high resolution analysis are too short.

Discussions in the paper in most cases apply a rather linear/sectorial approach. Essential challenges relating to the rapid changes in the cryosphere, however, concern interactions and integrated systems including humans and their infrastructure, especially in densely inhabited regions like the Alps. Fall, winter and spring snow plays a key role concerning subsurface thermal conditions and perennially frozen ground on more gentle slopes but less so in steep rock faces. The stability of steep/cold hanging glaciers on rock walls strongly depends on basal ice temperatures and related permafrost conditions in bedrock, an issue often ignored in the corresponding literature. Vanishing ice at the surfaces of rock walls can enlarge effects from rising air temperatures with respect to freeze/thaw cycles, frost weathering, permafrost degradation and rock fall activity.

> Response: this addresses more issues related to the cryosphere that will be handled below.
>
> Responses to minor comments:
> p.3 l.2: A figure has been added to the chapter
> p.3 l.3: We agree and added two corresponding sentences.
> p.4 l.6: Thanks for this hint. We added the corresponding time period.
> p.4 l.15: We agree and changed the wording accordingly.
> p.4 l.27: corrected
> p.4 l.28: We believe this fact is not important in this context and therefore did not include it in order to make the manuscript not any longer.
> p.5 l.15: corrected
> p.5 l.16: corrected

Section 2b
Response to Reviewer 1

I - Responses to the annotated version (pdf) :

P.5, l.19 : "This section would better have the same structure as 2.1 (observed and future changes). It should focus on essential aspects and international networks/concepts."

> Response: Done. As proposed by reviewer 1, we changed the structure of this section, including one section on « observed changes in glaciers » and one section on « the future of European glaciers ». For that purpose, we merged and shortened the initial sections 2.2.1 and 2.2.2 into one section 2.2.1: « observed changes in glaciers », and added a new section. For both sections, we highlighted physical mechanisms that explain the observed and future changes. We also send the reader to the other sub-section (3x) where the challenges to improve these estimates are already listed.

P.5, l.20 : "Probably first of all, glacier changes are a primary key indication of rapid and global climate change."

> Response: done, the sentence has been reworded: "Mountain glaciers are recognized as a key indicator of rapid and global climate change. They are important for water resources…"

P.5, l.24 : "Better eliminate: European glaciers are irrelevant concerning global sea level.
In case this has to remain, the latest overview concerning glaciers and sea level would be Marzeion et al. (2016): doi:10.1007/s10712-016-9394-y"

> Response: Done. Removed.

P.5, l.24 : "These are interesting papers but not really related to anticipating hazards related to glacier retreat". Hazards and risks related to the formation of new lakes would be a far better example, see Frey et al. (2010): doi:10.5194/nhess-10-339-2010 or Haeberli et al. (2016): doi.org/10.1016/j.geomorph.2016.02.009."

> Response: References have been added and the formation of new lakes included.

P.5, l.26 : "Mention replacement of vanishing mass balance glaciers, Findelen for instance, or Carturan (2016): doi:10.1017/jog.2016.107"

> Response: Done. A reference to Carturan et al. (2016) has been added.

P.5, l.27 : "True: Europe has by far the densest and most continuous observation network in the world. European glaciers have therefore been the backbone in the development of modern comprehensive monitoring concepts (GTN-G/GCOS)."

> Response: The sentence has been reworded: "Since 1894, glacier observations became internationally coordinated and continental Europe was the leader of this remarkable development."

P.5, l.31 : "These were point observations (better avoid the term „mass balance" for such point observations), not comparable with the glacier mass balance programmes, which started after World War 2."

Response: This section has been shortened and the observations on Clariden are not mentioned anymore.

P.5, l.33 : "Mention Storglaciären, Sarennes and Storbreen here rather than on lines 30/31."

Response: This section has been shortened and the observations on these three glaciers are not mentioned anymore.

P.6, l.5 : "Better „calibrating". Homogenizing is somewhat else (cf. Zemp et al. 2013, TC7)"

Response: The sentence has been removed in the new text.

P.6, l.8 : "Here or in the previous section, the comprehensive analyses of all glaciers in the European Alps by WGMS (1995, 2007) and Zemp et al. (2006) should be mentioned"

Response: Analysis of observed changes in Europe was addressed using various references. We have listed the most recent reviews of these changes.

P.6, l.9 : "Total volumes are also available - the uncertainty of the estimates is about plus/minus 20%."

Response: Done and added in Table 1.

P.6, l.13 : "Add „intermittently"."

Response: Done

P.6, l.21 : "Why is this part here? No future? Better integrate the essential aspects (NAO, temperature, albedo) into the previous section on observed changes."

Response: Done. Sections have been changed. Mechanisms are now detailed in the "observed changes" section.

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II - Response to the general comments:

Comment : « Snow and ice research in the Scandinavian mountains seems to be rather weakly represented and options offered by the explosive development of remote sensing capacities and new surveying technologies could be a stronger focus. »

Response: "Actual and future remote sensing capacities and new surveying technologies are addressed in section 3.X (Grand challenges)".

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Comment : "But what do we learn from this in comparison with, for instance, the repeated precision mapping of glaciers in the Eastern Alps since the end of the 19th century (not mentioned)?"

Response: In comparison to repeated glacier mapping at intervals of several decades, mass balance measurements provide insights into the response of glaciers to climate forcing at short (seasonal) temporal scales and therefore allow understanding the driving processes.

We however fully agree with the reviewer that the value of glacier inventories in glaciology also needs to be mentioned. However, no more text has been added considering: first, this information appears in the papers listed in Table 1 (the reference papers for the glacier surface area in the different countries). Second, a sentence already mentioned the interest of repeated inventories to estimate glacier retreat throughout the 20th Century "Repeated inventories showed a reduction in glacier area of 11% in Norway between 1960 and the 2000s (Winsvold et al., 2014), and 28% in Switzerland between 1973 and 2010 (Fischer et al., 2014)"

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Comment : "Already in 1894, glacier observations became internationally coordinated with the participation of the European countries being the backbone of this remarkable step. Later, several integrative treatments of all glaciers in the European Alps have been published, marking major steps in the development of worldwide glacier monitoring. The inventory analysis in 1995, for instance, was a pioneer effort elaborated on behalf of UNEP for estimating various physical parameters (including shear stresses, response times or thermal conditions, etc.) for all glaciers and provided the first reliable estimates of ice thicknesses and volumes. The comprehensive treatment in 2007 used the example of the European Alps for illustrating the integrated tiered monitoring concept developed for the Global Terrestrial Network for Glaciers (GTNG) within the terrestrial component of the Global Climate Observing System (GCOS in support of UNFCCC, not mentioned), i.e. at highest scientific and political levels. "

Response: Sentences were reworded to address the international coordination of glacier monitoring in Europe and to highlight the integrative value of long-term monitoring: "Since 1894, glacier observations became internationally coordinated and continental Europe was the leader of this remarkable development. Gathering the rich and unique records of observations in earlier centuries, including paintings, photography (Zumbühl et al., 2008), length-change measurements (Zemp et al., 2015), direct surface mass balance observations, repeated mapping of surface elevation or remote sensing observations of changes in surface state (e.g. snowline, albedo, debris cover), allowed integrative studies to assess extent, surface, ice thickness and volume changes of various glaciers. »

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Comment : "Integration of length and mass change data with the "dynamic fitting" concept (Oerlemans 1998; Climate Dynamics) within the framework of the international ICEMINT project among other integrative analyses (for instance, Zemp et al. 2006; Geophysical Research Letters) already showed many years ago with simple but physically sound models and techniques that the glaciers of the European Alps would largely disappear within decades even with moderate climate scenarios (see also Salzmann et al. 2012 concerning effects of a global 2_ goal; Environmental Research Letters). This is important to mention – especially concerning impacts/adaptation – because it documents that results from simple as well as complex model simulations concerning future glacier evolution in the European Alps have been available and robust for many years now (not everything is uncertain). In a similar way, information on long-term commitments concerning continued glacier shrinking due to delayed responses (Mernild et al. 2013, TC 7/5) should also be included, because it shows that it is most probably too late now to save more than small remains of the European glaciers. Such critical reflection about the relative importance and innovative input of the available scientific literature could again help with focusing on essential messages."

Response: This comment is now thoroughly addressed in section « The future of European Glaciers ». Projections of future changes are detailed in terms of the variety of models and approaches. The response of European glaciers even under a stabilized global warming of around +2°C is discussed. Suggested references have been cited.

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Comment : "Discussions in the paper in most cases apply a rather linear/sectorial approach. Essential challenges relating to the rapid changes in the cryosphere, however, concern interactions and integrated systems including humans and their infrastructure, especially in densely inhabited regions like the Alps. Fall, winter and spring snow plays a key role concerning subsurface thermal conditions and perennially frozen ground on more gentle slopes but less so in steep rock faces. The stability of steep/cold hanging glaciers on rock walls strongly depends on basal ice temperatures and related permafrost conditions in bedrock, an issue often ignored in the corresponding literature. Vanishing ice at the surfaces of rock walls can enlarge effects from rising air temperatures with respect to freeze/thaw cycles, frost weathering, permafrost degradation and rock fall activity. "

Response : This is an important comment, partly related to the field of permafrost research (see respective references to recent studies in that section). We have now added an additional paragraph on the thermal regime of glaciers and basal condition including new references and a description of processes. Cold glacier instabilities as a consequence of atmospheric and dynamical changes in the last decades are described in the section "observed changes".

Section 2c
Response to reviewer 1

Only one author (?) seems to treat permafrost (three illustrations, many citations are missing in the reference list).

> Response: we apologize for the missing citations – the reference list has now been updated.

Discussions in the paper in most cases apply a rather linear/sectorial approach. Essential challenges relating to the rapid changes in the cryosphere, however, concern interactions and integrated systems including humans and their infrastructure, especially in densely inhabited regions like the Alps. Fall, winter and spring snow plays a key role concerning subsurface thermal conditions and perennially frozen ground on more gentle slopes but less so in steep rock faces. The stability of steep/cold hanging glaciers on rock walls strongly depends on basal ice temperatures and related permafrost conditions in bedrock, an issue often ignored in the corresponding literature. Vanishing ice at the surfaces of rock walls can enlarge effects from rising air temperatures with respect to freeze/thaw cycles, frost weathering, permafrost degradation and rock fall activity.

> Response: We would like to thank the reviewer for the suggestion of additional topics to be included in this chapter and tried to now to better link the snow and the permafrost chapter. We think though that an additional focus on frost weathering/rock fall would be a bit outside the main focus of this chapter with its present aims.

Section 2d
Response to reviewer 1

A review or even "comprehensive assessment" about such complex matters as "the mountain cryosphere in Europe" cannot deal with everything but should at least mention what is being left out. In the submitted paper, lake ice with some long observational series and in cases heavy touristic use (St. Moritz, for instance) belongs to this category. Ice core drilling in cold high-altitude firn areas (for instance, Monte Rosa or Mont Blanc) should be mentioned as providing important information about pollution in industrial times and even climatic conditions during the Holocene. Mention should be made of cold miniature ice caps at lower altitudes (Oetztal iceman, Juvfonna in Norway, recent Ortles drilling) about cold/millennia-old ice now or soon melting away, indicating that the reduction in ice extent in European mountains may presently be exceeding variability ranges during pre-industrial times. Warming up and degradation of rock glacier permafrost which has persisted during the last about 10,000 years as documented in permafrost core drilling (for instance, Lazaun, South-Tirol) would also be an essential process characterizing the state of the mountain cryosphere. And how realistic are attempts to save glacier ice with white blankets or even artificial snow?

> Response: All comments have been integrated into the manuscript of the text, and missing literature has been added to the references section. The lake ice issue has been added to the meltwater hydrology section. (Cold miniature ice caps at lower altitudes, warming up and degradation of rock glacier permafrost, and the attempts to save glacier ice with white blankets or even artificial snow)

Section 2f
Response to reviewer 1

The contributions about the wide field of ecosystems in connection with snow, permafrost or glacier fore-fields appear to remain marginal if not random (two case studies from zoobiology, two illustrations).

Response: An attempt has been made in the revised version of the manuscript to better integrate these aspects into the overall objectives of the paper.

Section 3b
Response to reviewer 1

Concerning model approaches, needs and priorities for process understanding should be clearly discriminated from needs and priorities concerning impact assessments and practical applications. As an example, some short remarks on page 20, lines 25-29, refer to the results of an inter-comparison project concerning models for ice thickness estimates. Here, it could be mentioned that sophisticated flux-related approaches reflect our complex scientific understanding and are needed for sensitivity analyses but must be heavily tuned, etc. In practical applications, transparency, robustness, easy application or limited data requirements in simple stress-driven models (which need no excessive tuning and perform equally well) can be a serious advantage. In fact, realistic modeling of glacier bed topographies and DEMs without glaciers over large mountain areas (Linsbauer et al. 2009, for example) as probable future topographies has been a fundamentally important innovation. This step had been made possible by the combination of 3D approaches (already introduced by WGMS in the 1990s using the example of the European Alps) with digital terrain information. It opened the door for the emerging research field dealing with future landscapes in de-glaciating mountain chains and could well be one of the key messages of the review. This example illustrates that a systematic differentiation – who needs what? – may help with breaking down the extensive discussions into key messages of interest even for non-scientific circles.

> Response: Section 3.2 has been revised. My main comment here is formulated at the end of Section 3.2; In this section, the snow part should be condensed/shortened (mainly focusing on the challenges) at the benefit of some paragraphs on the grand challenges in modeling of glaciers and permafrost. There are completely missing and have other issues, challenges and future needs than modeling snow. I suggest including a few paragraphs, but don't feel really able to do that in a complete manner. Who could write a little paragraph here? C. Hauck (permafrost) and M. Huss (glaciers)?

This section is mainly about snow and provides a detailed discussion. Should it better be combined with corresponding parts about modelling glaciers and permafrost in a separate chapter on „modelling" rather than in a mixture of highly different things on „challenges".

> Response: We will consider merging the sections when we have received positive responses from all reviewers. This will be probably be done when we prepare the final draft.

Section 3f
Response to reviewer 1

Geomorphology (two to three authors?) mainly relates to hazardous phenomena but largely ignores landscape change with options for use but also new risks. General thoughts about uncertainty/communication (2 illustrations) are correct but not specific to Europe, mountains or the cryosphere.

Response: The main aim of the chapter - as the title states - is on geomorphic risks arising from melting permafrost, and this has been described in detail in paragraphs under 3f. Many of tehse processes are similar across the mountain ranges of the world and not specific to the Alps alone, as the referee states. We do not see, however, why we should not mention uncertainty/communication only because similar assumptions/observations can be made elsewhere. Landscape change was not the focus of this contribution, but we agree that some indication of some possible benefits, new opportunities could be mentioned, preferably however in the conclusion section rather than in chapter 3f where the focus is on hazards and risks.

Section 4
Response to reviewer 1

The conclusions of the paper elaborate on general aspects concerning data availability and communication but – surprisingly enough – say nothing concrete about the state of knowledge, the evolution or practical impacts related to the mountain cryosphere in Europe, nor do they provide specific recommendations for focused research. This leads to the impression that the contribution in its present state constitutes a rather preliminary "workshop report". Transforming it into a systematic, balanced, critically reflected and focused scientific review about the European mountain cryosphere with essential messages and concrete recommendations would need additional steps requiring deeper reflection about topics/ sources, more synthesis/weighting efforts, a clearer/more systematic structure and especially consensus-finding about key messages and recommendations for the future. The following thoughts (mainly on glaciers, permafrost, geomorphology/hazards and landscape change) may indicate some possibilities.

Chapter 4 could focus on special challenges; here an explanation would have to be given about the reasoning behind the corresponding selection of topics.

One of the most important topics, which should be included in a paper about the mountain cryosphere, is the rapid development of new landscapes – an important emerging field of research and climate change adaptation. Dealing with this topic necessitates a systems approach including socio-economic aspects. An example are the numerous new/future lakes which create opportunities for hydropower, tourism or water supply but also increase risks from impact/flood waves created by large rock/ice avalanches. The terms "hazard" and "risk" should thereby be clearly discriminated. Concerning hazards, a clear discrimination should also be made between processes/phenomena (avalanches, floods), which are mainly driven by short-term weather conditions and have a stochastic-type of temporal occurrence patterns on one side, and cumulative processes/phenomena (glacier vanishing, permafrost degradation, slope stability, landscape change), which now continuously evolve over longer time intervals. For the latter, the future will not only be different from the past but also from the present. This requires a specific and rather difficult hazard-prevention and risk-reduction strategy (scenariobased assessments including socio-economic aspects) to be developed and applied. A deeper understanding of future conditions in nature also requires consideration of different response characteristics related to the involved cryosphere components and the corresponding geo- and ecosystem factors. While glacier vanishing now is a matter of decades, permafrost degradation or slope destabilization can take centuries if not millennia to come. This means that European high mountain regions are experiencing a rapid transformation from glacial to periglacial landscapes with extreme disequilibrium conditions concerning slope stability, sediment cascades, or vegetation cover, etc.

Back to the conclusions, which may be the critical aspect concerning the value of the product: A number of bullet points formulating key messages and recommendations would help. They would document the necessary analytic reflection and synthesis process, thereby making the difference between a heterogeneous workshop report and a systematic, balanced, critically reflected and focused scientific review with a clear added value as compared to already existing modern reviews on mountains and the cryosphere. Specific remarks Specific remarks and suggestions can be found in the annotated file. The reference list is variable in style and largely incomplete (citations in the text not contained in the reference list are marked in pink). Careful editing is necessary.

Response: The lead authors of the manuscript make the same general response as for the replies to the comments relative to the introduction. The lead authors will attend to major changes in the conclusions of this Review Paper once the responses to reviewers have been accepted. Indeed, if further reviews involving additional or modified texts are requested of the author team, these changes will obviously have an additional impact on the way the introductory and concluding chapters are redrafted, and it is probably premature to attend to these at this stage of the review process. There are a number of important and interesting points raised in these comments that will be attended to when preparing the final draft of the Review Paper.

RESPONSES TO REVIEWER 2

May 3, 2017

Introduction
Response to Reviewer 2

A stronger, more integrated synthesis of what is known about current and future cryospheric change in Europe; what has already been resolved by many studies to date, and what are the key knowledge gaps.

> Response: This will be attended to (also see responses to Reviewer # 1) when the comments relative to the various chapters of the Review Paper have been accepted by the reviewers.

Section 2
Response to Reviewer 2

A section on elevation-dependent climate and cryosphere trends in the different regions might offer a good focus on a hot topic, providing a vehicle for integrative discussion and strategic (vs. sometimes ad hoc seeming) graphical additions to the manuscript. For instance, can changes in snow, temperature, precipitation, rock temperature, and glacier thickness be potted vs. elevation in Norway and the Alps? Integrating all available data.

> Response: Of course, a comprehensive comparison of elevation-dependent climate and cryosphere trends and integrating all available data would be a very powerful and interesting outcome of the paper. However, for all the mentioned fields like changes in snow, temperature, precipitation, rock temperature or glacier thickness (changes) the required observations and data are not (yet?) there. Nevertheless, this aspect could be an attractive new aspect in the conclusions, and the necessary respective comparative investigations a valuable stimulation of new research. We have included a respective hint in the final chapter of the paper.

Section 2a
Response to Reviewer 2

European and more regional-scale maps and trend values (tables) of recent changes in snow, permafrost and glaciers, and perhaps also projected changes; right now there is discussion of all of this, but more on a case study basis, and it is difficult to infer general conclusions.

> Response: To compile such a table is currently almost impossible since existing studies analyze the snow cover changes using, for example, different time periods and altitudes ranges. A Eurpoean scale analysis would require a database of snow observations containing data for all relevant areas so that the changes can be assessed using common time periods and elevation bands. A table regarding future changes would be more realistic, but still has the problem with different altitudinal ranges and new problems concerning different emission scenarios and different climate models.

Also the explanation of stable snow cover due to increasing Eurasian snow cover does not make sense; what is meant by Eurasian (does this include the Alps), and can the geography and atmospheric process(es) that connect the proposed links be more specifically explained?

> Response: The original sentence was misleading since we missed to mention that only the Eurasian snow cover in autumn is increasing. We corrected this now. In this case Eurasia does not include the Alps. We give two references since the atmospheric processes are complex and under debate.

Is it still true, and generally accepted, that there has not been winter warming over the Alps or 'large areas of the northern hemisphere' since the 1990s? This seems surprising. Also not consistent with some of the narrative p.11, l.14, discussion of warmer winters giving increased winter rainfall runoff.

> Response: Yes it is still true and not in contradiction with p.11 l.14, because the non further increasing temperature are just valid for meteorological winter month DJF and for higher elevations (roughly above 1000 m

p.4, ll 2-6, discussion of Alpine snow cover changes. Some things don't make sense as described. For instance, it does not seem reasonable that SWE and snow depth will decline while snow-covered area does not change. In winter perhaps, but spring and summer snow cover will surely decrease if there is a thinner snow pack.

> Response: We agree this was misleadingly written. We rephrased and replaced the sentence to the end of the paragraph.

Section 2b
Response to Reviewer 2

p.18, nice discussion of the uncertainties and challenges of spatial scale; I was left wanting though, for how to bridge local to catchment and RCM/GCM scales when it comes to observational validation at the larger scales. Some perspective and thoughts here would be welcome – what is needed to give e.g. SWE or snow hydrological datasets at the larger scales, for model validation?

> Response: Very good point. Recent sensing technology (e.g. ALS and TLS) can provide spatially resolved data of SCA and snow depth. Missing elements (data) related to the snow mass balance are spatially distributed snow density, and liquid water content data as well as local wind data for modeling snow transport, drift, erosion, redistribution etc. These quantities can be modeled, but corresponding data sets should be available for the validation of such model outputs. Related to snowpack energy balance, distributed snow albedo data would be desirable. Limitations in spatial resolution in satellite remote sensing data do not allow for high resolution surface albedo suitable for model validation. A promising approach here could be the use of drones or other UAVs equipped with radiometers and multispectral cameras. Current studies give evidence that this field is rapidly evolving. --- This information has been added in Section 3.2 of the manuscript.

p.19, l.14, out of curiously, what percentage of the European landscape is above 3000 m? Somehow I guess it is not much more than 1%, so I am curious how underrepresented these high elevations are. p.19, l.21 “are” crucial

> Response: The reviewer is right, there is only a small areal fraction of the European landscape with elevation >3000m and the majority of data originates from lower altitude stations. However, if we consider the areal fraction in which cryosphereic processes are relevant, in particular permafrost and perennial snow during the summer, this percentage of this fraction is more important and observations from these areas are crucial for the study of the state and processes of the cryosphere. --- A corresponding sentence has been included in Section 3.3

p.19, l.29, “has allowed a better understanding of”
p.21, l.17 “are also a subject”
p.22, l.21, “focus on” p.25, l.2, “built”
p.25, l.13, “concepts”

> Response: All suggested edits have been attended to in the revised manuscript.

Section 2c
Response to reviewer 2

European and more regional-scale maps and trend values (tables) of recent changes in snow, permafrost and glaciers, and perhaps also projected changes; right now there is discussion of all of this, but more on a case study basis, and it is difficult to infer general conclusions

> Response: There are currently no regional scale maps or trend values for current and projected permafrost change in Europe - partly, because there is only an insufficient number of borehole data available, but mainly because the heterogeneity of the mountain regions is so large, and permafrost depends strongly on surface and subsurface characetristics (e.g. fractures/unfractured rock, fine/coarse-grained sediments, porosity etc), microclimatic factors (energy balance of the whole atmosphere/active layer system, convection in teh active layer etc) in addition to classical topoclimatic factors (elevation, aspect, slope angle). In addition, the heterogeneity of teh snow cover which may vary spatio-temporally on very small scales is an additional influencing factor, as it effectively insulates the permafrost from atmospheric influences. This is why case studies dominate teh scientific analysis - we include these statements now in section 2.3

Section 2d
Response to reviewer 2

Figure 5. Is the glacier runoff on this chart the specific runoff, i.e. mm/month per unit area of glacier cover? Or is it normalized over the full catchment? Probably the latter, given the values. In which case, the overall runoff reduction by end of century is less than I would have expected, given the dashed lines for the control period, especially for RCP4.5. Is it because there is some extra summer rainfall helping out, or are the deglaciated basins giving new lakes that help to reserve and release meltwater through the summer months? Is the latter process included?

> Response: The runoff is given as mm (= l/m2) per month and accounts for the entire catchment as defined by its gauge. The dashed lines indicate runoff from the respective glacierized area only (explanation in the figure). The magnitude of the modelled regime shift is due to the relation between the runoff originating from glacier melt compared to runoff originating from precipitation. This relation is catchment-specific.
>
> (We have replaced fig. 5 with a newer version, according to our newest simulations, and considered all comments to improve it accordingly).

Section 3a
Response to reviewer 2

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> Response: All suggested edits have been adopted in the manuscript.

Section 3f
Response to reviewer 2

p.26, Figure 8 discussion. Differences are shown in mm/day and the graphic is interesting. For context, is it possible to give a value of the average DJF and JJA precipitation for one or two high-elevation examples, perhaps in Switzerland – i.e. is a difference of 5 mm/day equivalent to 30%, 50%, 80%, etc? Some idea of the scaling would be helpful to assess whether the magnitude of differences is significant. Also, are these three datasets covering the same period? The Swiss-Italian border seems to show up prominently here. Is this related to altitude, or national measurement protocols? It might be interesting to make a plot of the precipitation difference vs altitude, at least for the central European subset of data. Norway might have its own story, but this is also worth a look.

p.26, Figure 9 could be better discussed as well. The figures show precipitation anomalies – is this the 'error' vs. observations? Against what observational dataset? Again, some idea of the scaling would be helpful, e.g. and average wet or dry bias of * % for a given region. Perhaps consider again plotting the anomaly vs. altitude. I would love to see some discussion of the processes involved in wet vs. dry biases in different models, while appreciating that this may be beyond the current scope

p.26, l.34, "can provide adequate data for modelling atmosphere-cryosphere systems" – I think this is arguable – the biases over mountain regions are still huge with nested RCMs; processes here simply aren't resolved

Figure 8. Note in the caption that these are three different observational products, and also the time frame for each of these (is it the same, e.g., 1979-2014?)

Figure 9. These precipitation maps are expressed as anomalies: with respect to what? Please state in the caption.

> Response: The purpose of the Figure 8 is just to higlight the differences of the OBS datesets (we could make the figure again in percentage to make him/her). The period of the observation are coincident and for such kind of paper any more in depth analysys it would be out of context. There are many other papers cited in the references that deals with this problem.
>
> Figure 9 shows the precipitation anomaly fo each ensemble member compared to the ensemble average. No observations are involved in the figure.

RESPONSES TO REVIEWER 3

May 3, 2017

Introduction
Response to reviewer 3

General comments: My major concern is: "What can the reader learn from the paper as it stands now?" "What is the benefit of the paper in comparison to the list of individual papers summarized/extracted there"? My expectation is that from summarizing the previous studies/papers new insight/findings should result. This is what I clearly miss in the paper. It is much too much a summary of previous studies without extracting new information and introducing innovation.

If the aim of the paper is to deal with mountain cryosphere your discussion has to cover more than only glaciers, permafrost and snow, such as lake- and river ice or cave ice. At least you have to make clear that (and why) you exclude these parts of the mountain cryosphere (because of whatever reasons).

> Response: The lead authors of the manuscript will attend to major overhauls to the introduction and conclusions of this Review Paper once the chapter revisions / responses to reviews have been accepted by the reviewers. Indeed, if further reviews involving additional or modified texts are requested of the author team, these changes will obviously have an additional impact on the way the introductory and concluding chapters are redrafted, and it is probably premature to attend to these at this stage of the review process. There are a number of important and interesting points raised in these comments that will be attended to when preparing the final draft of the Review Paper.

Section 2
Response to reviewer 3

Further, I do not really see the benefit of sections 2.4 to 2.6 for achieving the aim of the paper. These chapters make the paper longer and leaves the reader alone with question why exactly these topics were selected as examples (there are several things left out with respect to e.g. the cryosphere and ecosystem functioning or the cryosphere and hydrological impacts, which are important). For your discussion of past, current and future issues of mountain cryosphere the detailed description of impacts is not needed (at least not as shown in the paper now). If you want to show issues/challenges coming from the impacts you have to make this more explicit

> Response: Chapter 2 is on the trends in the cryosphere, and their impacts. We clearly anounce this in the introduction of the paper. Accordingly, the author team developed a list of current/future trends and respective impacts, which is reflected in the structure of chapter 2. We do not claim these phenomena to be complete, but consider it to be a collection of the most significant. The respective changing runoff regimes in glacierized catchments are a effect of climate change and of high importance for the water management along the mountain rivers. For more completeness, we have included the lake ice break-up topic. Fiinally, we have modified the heading of chapter 2.4 to make clear that we relate to the melting of snow and ice as important components of the mountain cryosphere here.

Section 2a
Response to reviewer 3

In section 2.1 the past and future evolution of snow is described. Surprisingly, there is no single figure on snow development in the paper. However, snow could be seen as the key component of the cryosphere, highly relevant not by itself but also as a key forcing of changes of glaciers and permafrost through direct impact and various feedbacks. Thus, at least one figure on changes of snow parameters as well as their spatiotemporal evolution would be key for a paper on the status/changes of the cryosphere.

> Response: We have added a map showing the spatiotemporal trends in spring snow water equivalent for the European Alps. This particular figure was adapted because the data it is based on several countries and a long time period unlike most other studies that focuses on smaller regions and shorter periods.

Chapters covering both the glacier changes and the changes of snow cover are rather descriptive without real understanding of underlying mechanisms, beside the impact of NAO. However, the discussion of the impact of the NAO on snow and glaciers in the Alps and Scandinavia remains general. This appears bit "old-fashioned" approach and leaves the reader with simple findings which are already well known.

> Response: We agree and added some information about the influence of the AMO.

Section 2b
Response to reviewer 3

Comment : « Chapters covering both the glacier changes and the changes of snow cover are rather descriptive without real understanding of underlying mechanisms, beside the impact of NAO. However, the discussion of the impact of the NAO on snow and glaciers in the Alps and Scandinavia remains general. This appears bit “old-fashioned” approach and leaves the reader with simple findings which are already well known.”

> Response: Sections have been completed to detail physical mechanisms of the observed and future changes, causes and consequences.

+++++

Comment : Figures 1 and 2: Would be good to know where the measurements of glacier length/mass balance and borehole temperatures are located in Europe.

> Response: Done. Countries names have been added in Figure 1.

Section 2c
Response to reviewer 3

Heading 2.3.4 Modelling (please explain with respect to what?)

> Response: Changed to "Permafrost evolution modelling"

Figures 1 and 2: Would be good to know where the measurements of glacier length/mass balance and borehole temperatures are located in Europe.

> Response: I suggest to include a new figure (Figure 1 new) where all major sites mentioned in the text are located - map of Europe with an insert for the European Alps. Better than have individual maps for all variables (glacier, permafrost boreholes etc)

Figure 4: Figure captions has to be increased in size.

> Response: Figure 4: Figure subtitles and axis titles have been increased in size.

Section 2d
Response to reviewer 3

In chapter 2.4. you are discussing changes in hydrology, however in fact you are discussing changes in stream flow (amount) only.

Figure 5: The figure needs to be simplified and more generalized for a review paper. What is the meaning of dashed lines?

> Response: We have accordingly adopted the heading of the chapter, re-arranged the introductory part and included an explanation of the importance of meltwater to streamflow, and its meaning for the downstream population. This also represents the necessary connex to chapter 2.5. We also have added the lake ice break-up issue, and we have updated fig. 5 from our newest simulations.

Section 2f
Response to reviewer 3

Figure 6 and 7: You have two figures for this rather specific impacts but no figure on snow. My advice is to skip at least one of these figures.

Response: This comment will be taken into account in the updated draft of the manuscript

Section 3
Response to reviewer 3

Generally, the paper is too descriptive and without an integrating approach (e.g. synthetizing the findings from snow, glaciers and permafrost to new information). My impression is also, after reading the paper, that your main aim was not achieved. The challenges that need to be addressed in future research remains open. For example, you mention data-issues as a core challenge. However, your conclusion on this important topic are rather general and un-specific and implies no in-depth treatment of the topic.

> Response: I suggest a new Section 4 summarizing the grand challenges of Sec.3, or alternatively, include this summary in the Conclusion section and call it "Integrative summary and conclusion" or similar; actually, I think the latter is the better option as the paper is already very long… in this part we could address most of the 3 referees' comments regarding the principal message and aim of the paper. However, I think that this will take another iteration once we have all the requested specific revisions implemented. At the moment, reviewers are right, the paper is still a bit a patchwork...

Section 3a
Response to reviewer 3

Section 3.1 deals with data issues for cryosphere observations. Though very important, this section is clearly too vague. There is neither distinction between ground observations and satellite products/data nor a clear concept what is needed in the future. Without a clear concept the requirement for improving cryosphere observations is weak and not applicable for the reader. What about guidelines/best practices in cryosphere observations? Are there needs for standardization of measurement? Could existing homogenization methods be easily adopted for cryosphere variables? There are clearly more questions to be addressed under this chapter. Consequently, either the authors deal with the subject of data issues in more detail and extensive or they skip it. The current version is without real value.

Response: The manuscript is not consistent w.r.t. British/American English spelling. Should modify according to TC requirements. Also the references have to be made consistent and TC compliant but this is technical work at the end for when the list is not changing anymore.All three reviewers acknowledge the importance of this section but, the reviewer is right, it is fairly general and to some extent unspecific to the cryosphere. However, we are convinced that data issues are one of the big challenges (and obstacles) in cryospheric science for the reasons explained in the manuscript, and therefore want to maintain this section. We appreciate the valid remarks and revise the section accordingly (not yet included in revised manuscript):

Section 3b
Response to reviewer 3

Under chapter 3.7 (uncertainty) the comparison between different precipitation data sets in Europe as well as the model uncertainty for precipitation from RCMs is shown. This again comes a bit unexpected. Why is this relevant in the context of “changing mountain cryosphere”? Is it useful to discuss this here, with a few sentences only? This needs to be made more clear and better described.

Figure 9: What is the temporal reference of the figure?

> Response: The purpose of this section is to make the reader aware of both observations and models uncertainty and therefore we need to take this into account when we validate the model results in regions like mountain where these uncertainties are even bigger. Even more important we need to take this into account when we deal with climate change driven signals.
>
> The years are now added in the caption of Figure 9.

Section 4
Response to reviewer 3

My suggestion is to clearly rework the paper and try to make take home message much more evident for the reader. Such take home messages could come from e.g.: - Added value from putting together the information from all different components of the cryosphere (snow, permafrost, glaciers) and try to derive extensive findings, e.g. you could describe how mass balance changes of glaciers fits with changes of summers now at high mountain sites and how this contradicts with winter snow. Which seasonal climate sensitivity was observed for glaciers-, permafrost change and how has it changed with time since the begin of observations. How do snow trends fit to mass balances of glaciers? - Added value from putting the focus on Europe thus interpreting the findings from Scandinavia/Alps/Pyrenees for an European perspective of understanding of climate change. Do we have gaps in the observations with respect to spatial coverage? For a comprehensive view of change of mountain cryosphere I would very much like to see also other time series of changes of the cryosphere such as freshwater ice or ice in caves.

> Response: The lead authors of the manuscript make the same general response as for the replies to the comments relative to the introduction. The lead authors will attend to major changes in the conclusions of this Review Paper once the responses to reviewers have been accepted. Indeed, if further reviews involving additional or modified texts are requested of the author team, these changes will obviously have an additional impact on the way the introductory and concluding chapters are redrafted, and it is probably premature to attend to these at this stage of the review process. There are a number of important and interesting points raised in these comments that will be attended to when preparing the final draft of the Review Paper.