POINT-BY-POINT RESPONSE TO REMARKS BY THE REVIEWERS

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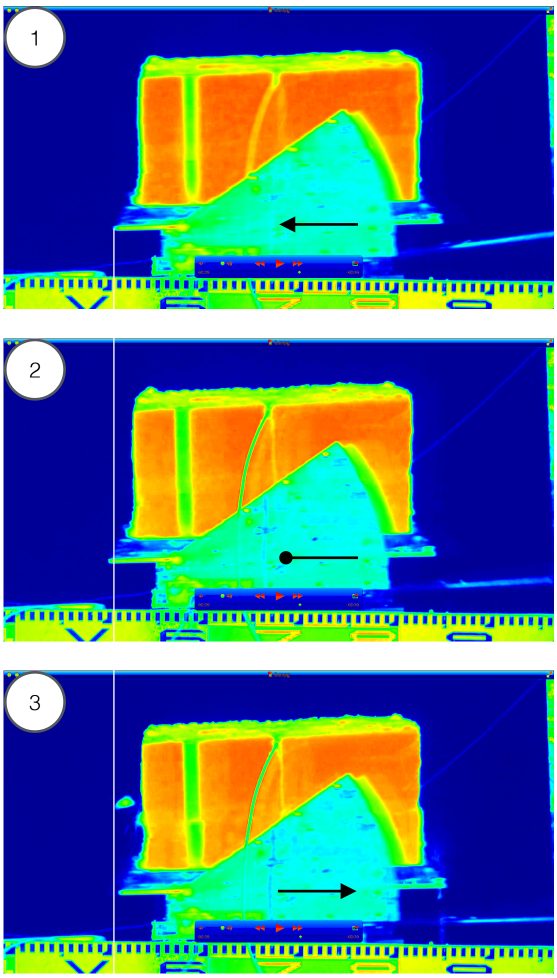
First of all, we are grateful to the Reviewers for criticisms and comments. We have taken advantage of this opportunity to revise the paper by addressing or answering the provided remarks as explained in our responses below. EAP also thanks his family for providing him with conditions and financial support necessary for working on this revision.

#	**Comments from the Reviewer #1**	**Authors' response and changes in the manuscript**
1	In the paper, a method is presented to evaluate weak layer constitutive parameters based on a combination of snow fracture experiments and finite element modeling. The approach presented in the paper is interesting and provides information which would not be available solely based on experimental results, and the paper addresses the important issue of how to model weak layer failure, which, despite various studies, is still mostly an unresolved issue. As such, the paper should be considered for publication in NHESS. However, major revisions are required before it can be accepted for publication. Overall, the paper requires extensive restructuring and rewriting to make the paper more readable, and the authors need to do a better job to justify some of the assumptions made in the model.	We substantially amended the paper to account for reviewers' criticisms. During the two stages of the review we attempted to follow comments by the three referees and the editor. At the initial stage, the major point (accompanied by several pages of minor remarks) by the two referees and the editor was to reduce the size of the paper and to remove repeating statements for improving the readability of the text. We carefully followed this suggestion and reduced the main text at least by 3 pages. Following this revision, the Referee #2 has not provided any additional criticisms, Referee #3 pointed multiple technical corrections and the Editor wrote that 'the paper reads well'. Together with the present corrections, modification of 6 figures, clarifications, and substantial shortening of text (with one figure removed) we hope the paper was improved compared to the version submitted 7 months ago.
2	Specific comments: First and foremost, the authors should use the term fracture throughout the paper and not rupture. Rupture is for soft materials - a rubber hose or a blood vessel can rupture - while fracture is for brittle materials.	We replaced rupture by 'failure' throughout the paper.

3	Several assumption were made to build the FEM model and the authors should clearly state what these are and why they were made. Some of the assumptions are based on experimental results. Specifically, based on high-speed video recordings of the experiments the authors state that there was no discernible collapse of the weak layer during fracture and that the snow below the weak layer behaves like a rigid oscillator. Both these assumptions are crucial as they allow the authors to omit the lower block from the FEM simulations and treat the weak layer as an interface. I find it hard to believe that a 1-2 cm thick artificial weak layer consisting of dendritic snow with a density of 100 kg/m3 would not collapse during fracture, especially since collapse in low density storm snow has been documented in field experiments (Bair et al., 2012). Given the importance of these assumptions, it is somewhat surprising that no quantitative experimental results are shown to substantiate these claims, especially since those results are also not presented in Podolskiy et al. (2010). 	Initially it was stated in the 'Objectives and Scope of the Study' that assumed failure criterion is one ingredient among others, like stauchwall effects, post-peak-softening, fracture propagation, or normal collapse. In our tests, we were not able to recognize any significant normal collapse. As referee noticed, this experimental result was not presented in Podolskiy et al. (2010), however, a short mentioning of image analysis attempt was published in Podolskiy (2010, p. 154). We added a reference to this evidence into the paper. Therefore, we, as designers of the model, do not take into account such process as normal collapse. Of course, this choice is also motivated by the absence of validated constitutive law including collapse for weak layers. Incorporation of this into the model could constitute a further refinement. Given that we are asked to reduce the size of the paper and were suggested to remove most of detailed discussion about assumed parameters, we do not consider that more information is necessary in the manuscript. Since our replies become an archived document, we use this opportunity to provide some examples here. As it was stated in the paper failure occurs at the point of maximum acceleration, when the platform changes its direction after a full stop. The presented 3 frames (with false colors to ease comprehension) illustrate the following examples of a horizontal test: (1) just before the failure at the final approach (from right to left) to a full stop, (2) a failure with a starting shear displacement, (3) backward trajectory of the platform (already more than 1 cm away from the point of a changed direction of movement) with inertial brushing of the upper block over the lower one. As previously noted, no significant normal collapse was observed during failure (2), which even if missed due to insufficient video quality, we safely limited to less than 1 mm. The erosion post-failure stage (3), corresponding to two discrete blocks and driven by inertia, which literally smashes the upper block into the lower is clearly beyond the scope of the model. Eroded snow is noticeable at the left part of the sample, which is simply 'frictioned' away from the failure plane up to 1 mm of depth. Because due to inertia the right side of the upper block is lifting, such erosion is hardly observed there. Finally, we have to remind that standard gravitational load during field tests with a saw cut (e.g. Bair et al., 2012) has little to do with the present experiments, because normally directed gravity is not comparable to much stronger horizontal inertial effects dominating in our study.

3	[continued; see above]	As explained in the paper (Section 3.2), we find that consideration of an underlaying block of snow as a rigid body is a reasonable assumption. Similar rigid behavior was observed or used in multiple studies to some of which we provided references (Gaume et al. 2012, 2013; Reiweger and Schweizer, 2010; van Herwijnen et al., 2005, 2010). Furthermore, in the section about weak layer stiffnesses Ks&Kn (which we were suggested to remove; remark #47), it was verified that if the layer below the upper block has too soft equivalent of Young modulus, such soft cushion would correspond to unrealistically large displacements of the upper block during vibration. Such displacements were not observed, suggesting that nothing below behaves as soft material. Given that sensitivity analysis of elastic properties of the upper block did not effect failure properties, we consider it safe to assume a rigid behavior for the lower block.
4	At the very least, the authors should provide one or two videos as supplemental online material. However, I would strongly urge the authors to include image correlation analysis to provide experimental evidence for their claims.	Image correlation analysis requires a dedicated study and deserves a full-length original research paper, like any previous PIV-based study (e.g., Reiweger and Schweizer, TC, 2013), which has to be specifically designed and to have sufficient amount of markers. We consider that the arguments provided above (reply #3) are sufficient to support our modeling assumptions.

5	In the FEM model, the weak layer is modelled as an interface with zero thickness, characterized by a normal and shear stiffness and obeying a Mohr-Coulomb failure criterion. As the oscillation frequency increases, stresses within the sample increase and progressively more nodes satisfy the failure criterion (Figure 7). Sample failure is then defined as the instant when the stress in all nodes exceeds the strength. However, prior to this instant, when the stresses are removed, no flaw remains. This assumption is not clearly stated in the methods section and only first addressed in the results section (page 4547). Furthermore, this assumption is not justified as it is well known that snow behaves as strain-softening material, as also assumed in Gaume et al. (2013). Thus, the authors need to provide more convincing arguments for their assumption, or show that including a more realistic behavior for the weak layer does not influence the results in any appreciably manner. For instance, I would expect that if nodes which fulfill the failure criterion are allowed to fail, and stress redistribution is taking place, the elastic properties of the snow block above the weak layer would have greater influence on the obtained results.	Because the size of our system is too small to observe stress redistributions (the characteristic length associated to stress redistribution by slab's elasticity is way larger than our system length, \Lambda~1m) elastic redistribution effects are of no importance compared to edge effects. In our case progressive failure is essentially driven by external load, and not by stress redistribution or critical propagation. In other words, the size of potential remaining flaws is always smaller that critical length necessary for crack propagation. The justification about failure definition was described in Sections 3.2 & 4.1.5. Please, also note that Gaume et al. (2013) was focused on a larger dimensions (tens of meters). At the developing stage of the model, the strain-softening constitutive law from the paper by Gaume et al. (2013) was unsuccessfully applied by the authors who worked on both papers simultaneously because of tensile forces in the interface that the model did not support. We mentioned in the paper that our approach was needed since it allows tensional strength of the interface (Sect. 4.1.3). For similar reasons, even in work by Gaume et al. (2013) an artificial boundary condition was introduced to avoid any tension. Furthermore, we have to point out that reference to snow as strain-softening material makes sense only in compression (i.e. in a shear-box tests or for an avalanche release under slab’s self-weight), in tension, however, it appears much less relevant. Therefore, direct analogy between the work of Gaume et al. (2013), which focuses on more common for literature self-weight-induced stresses, is not possible.
6	Overall, the paper is also a pretty hard read. It is rather lengthy, sometimes repetitive and somewhat scattered. The authors should restructure the paper as well as shorten some sections. For instance, section 2 describes the objectives and the scope of the study, while section 3.3 again describes the scope of the study. Clearly, the writing can be more to the point and compact.	The size of the paper was significantly reduced (by at least 6 journal pages) and restructured there possible. The Section 3.3 was removed as well as the previous Fig. 13 and about 14 bibliographic references. We hope that by following the Referee’s remarks the readability of the manuscript was further improved (please, see also our reply #1).
7	Technical corrections: I have provided more detailed comments in a separate annotated pdf file.	Wherever possible we attempted to make corrections according to remarks, which we had to extract manually from the Referee’s document and listed below for everyone’s convenience and in order to satisfy regulations of the journal.
8	4526: Abstract rewrite after you made the revisions to the paper.	The substantial revisions we brought to the paper did not change the main message of the abstract, which informs the reader about the most important points of the paper. Hence, we re-wrote it for improving clarity.

9	4526: Introduction You should also describe the experiments in the introduction since they are already published in Podolskiy et al. (2010). You can move the first paragraph of section 3.1 to the introduction.	Please, note that the experiments were introduced in Section 2 ('Objectives and scope of the study'), and in our opinion should not overload the Introduction. Furthermore, Section 3.1 gives technical details, which are too specific for the Introduction, and therefore should be placed separately.
10	4527: to investigate ~~the following~~	Removed.
11	4528: may be roughly classified	Corrected as: 'may roughly be classified'
12	4528: This is a rather poor argument to say that you treat the weak layer as an interface. There is absolutely no reason why different weak layers should fail in different ways. Furthermore, all experimental evidence, on various types of weak layers, including new snow, facets on crusts, weak interfaces, has always shown that weak layers collapse when they fracture. While it is clear that the parameters (strength, residual friction, fracture energy etc.)of different weak layers will exhibit a broad range of values, there is no reason why the physics of fracture should be different. Just state the due to computational difficulties it is much easier to treat the weak layer as an interface. You can also state the paper from Gaume et al. (in press) which shows that it makes little difference which method you use to treat the weak layer (anti-crack, shear, Mohr-Coulomb with strain softening).	We deleted the corresponding part.
13	4528: Ice lensens, melt-freeze crusts and wind crusts are not weak layers. The weak layer is the poorly bonded snow either above or below the crust.	Here we did not call these as 'weak layers'. In order to avoid any ambiguities, we replaced 'weak layers' by 'failure related surfaces' in places which were not removed (see #12).
14	4529: I would merge section 3.3 and this section together and significantly shorten this.	Done as recommended.
15	4529: Firstly… Secondly	Corrected to 'First', 'Second'.
16	4529: ~~we analyse the experiments. These experiments were one of the first cold laboratory tests with snow "sandwich" samples, allowing study of the mechanics of weak layer dynamic failure. Complex variation of stresses and normal pressure in particular provided a unique dataset for investigating performance of the assumed failure law under highly variable conditions. In particular, as in Chiaia et al. (2008), we are interested to test the importance of including normal stress dependence in the failure criterion.~~	Removed as indicated.

17	4530: propagation ~~or possible~~ normal collapse; Heier-li et al., 2008).	Corrected to 'and'.
18	4530: This can all be moved to the discussion	Moved as suggested.
19	4530: I would remove this, but that's a personal preference.	Removed.
20	4531: this should all go in the introduction	As it was already explained in our reply #9 these are technical details, which are too specific for the Introduction. This part was shortened.
21	4532-4533: move to methods	We thank for this suggestion, but prefer to leave it in this specially dedicated Section 3.1. Because the main methods of the present paper are numerical, and all this is a background about previously published materials, which, for example, can be considered as an equivalent to 'Study area/Data' sections of some field-based papers, before going to Methods.
22	4533: You can move this section to methods and shorten it.	Please, note that for the same reason as shown above, we prefer to leave this here - in order to avoid mixing of the background with the numerical approach as much as possible.
23	4533: you need to provide experimental evidence for this, based on image correlation analysis of the video	Please, see our previous replies #3&4. Here we just repeat, that image correlation analysis of the video presents a substantial separate work, which by itself, without any modeling served as a basis for many full-length specifically focused papers on snow.
24	4533: Again, you need to provide experimental evi-dence for this, based on image correlation analysis of the video	Same as above.
25	4534: this should go to section 4.2 model description	Moved as advised.
26	4534: move to section 2 and shorten	Text of this Section 3.3 about Mohr-Coulomb law was moved to Section 2, and all about failure mode to Section 6 ('Discussion').
27	4536: You need both experimental methods (parts of section 3.1 and 3.2) as well as numerical meth-ods. The numerical methods section should be restructured so that the reader can clearly fol-low what the assumptions are and what the val-ues of the parameters are.	As previously explained (please refer to our response #21), we prefer not to mix numerical methods of this paper with previously published experimental information, which is not a method of this paper and serve as a background. Such step will only overload this section. To clarify this, we have changed the title of Sect. 4 'Methods' to 'FEM modeling'. For easier comprehension of parameters we provided Table 2, which allows the reader to follow the assumptions and values. Other structure related remarks are answered below (e.g., #37).

28	4536: ~~In regard to the differences with other available programs (Podolskiy et al., 2013), we note that Cast3M is open-source software, which allows modifications to be made to the source code.~~	This sentence was completely removed.
29	4536: ~~(Podolskiy et al., 2010b),~~	Removed.
30	4536: and ~~no large~~ strains.	Corrected to ‘small strains’.
31	4537: ~~higher number of~~	Corrected as suggested.
32	4537: ~~For representing~~ the weak layer of the “sand-wich” samples ~~we treat it~~ as an interface. ~~The interface is~~ modeled by joint elements with four nodes (JOI2) ~~but~~ zero thickness…	Corrected.
33	4537: The “lower” part of the joint (1A0–2B0; Fig. 1c) is fixed to the bottom boundary, meaning that _^_ vertical and horizontal displacements ~~of this part of the joint are forbidden~~ relative to the boundary. However, the lateral and surface boundaries ~~of the rest of the system~~ are not restricted, thus allowing free deformation.	Corrected.
34	4537: ~~Therefore, these conditions are both compara-ble to those of a snow block frozen to the plat-form.~~ - not really, because you still assigned properties to the interface (stiffnesses and mohr-coulomb criterion) very different from a frozen contact.	Removed
35	4537: We note that the simulated geometry requires half as much computational time as ~~it do~~ if the lower block is included. ~~Furthermore, as it will be shown (Sects. 4.2.2 and 4.2.5), by introduc-ing interface stiffness (which may be seen as equivalent to putting the sample on an elastic cushion instead of a rigid plate) and making sensitivity to a wide range of values, it is possi-ble to verify if our assumption is reasonable. The stiffness was found as not playing any im-portant role in the key quantities controlling interface failure process (Sect. 5.3). In view of this simple observation it is quite obvious that the assumed model geometry does not control failure.~~	Removed.
36	4537: the interface should be described in a separate section.	Done.

37	4537-4538: "A choice of material properties of the block (i.e. Young's modulus, Poisson ratio) will be considered below (Sect. 4.2.5). Sensitivity tests to Young's modulus, E, Poisson ratio, , and viscosity, , will be shown in Sect. 5.3." - Move section 4.2.5 Young's modulus and Poisson ratio here and shorten them significantly	The indicated sections were moved and significantly shortened.
38	4538: A new section on the interface should start here.	Done.
39	4538: ~~of them~~ (tensile strength, st, and cohesion, c~~)~~ depending	Removed as indicated.
40	4538: "… tension stresses ~~(as it will be illustrated later)~~. Additionally to failure criterion, for joint elements we specify values of shear and normal stiffness, Ks and Kn, which control strains of the interface ~~(more details are provided in Sect. 4.2.5)~~." - move section 4.2.5 shear and normal stiffness of the interface here and shorten considerably	The text in brackets was removed as suggested. Section 4.2.5 was deleted and its most important information was incorporated here as advised.
41	4538: ~~the occurrence of total~~ sample	Removed as indicated.
42	4539: ~~Against the above-mentioned background and the size of specimens (Sect. 3.3), the implemented approach~~	Replaced by 'This'.
43	4539 You need to clearly state that when the stresses are removed, that are no flaws remaining, as you do on page 4547	We repeated this information in this place and in Section 3.2 as suggested.
44	4539: Having never done any FEM modelling, this seemed very odd to me, so you need to describe that this is a numerical necessity.	We just added that gradual application of gravity is made in order to avoid numerical instabilities.
45	4541: Move to section 4.2.2 Also, you can really shorten this section to just a few sentences. Just state what value you take for E, provide some references and state that the sensitivity tests (section 5.3) showed that results are not very sensitive to E.	This section was deleted and the values were mentioned in section 4.1.2.
46	4542: Move to section 4.2.2 Also, you can really shorten this section to just a few sentences. Just state what value you take for nu, provide some references and state that the sensitivity tests (section 5.3) showed that results are not very sensitive to nu.	Similarly, this section was deleted and the values were mentioned in section 4.1.2.

47	4542: Move to the new section 4.2.3 on interface properties. Again, you can really shorten this. Just state that there are hardly any experimental data on weak layer stiffness values. You should cite the only measurements by Föhn et al., 1998: Mechanical and structural properties of weak snow layers measured in situ. Then state the values you use and note that the sensitivity study showed that the Ks and Kn values have little influence.	This section was also completely removed and the values were mentioned in section 4.1.3. Citation added.
48	4543: This should all be moved to section 4.2.4 and 4.2.1 and shortened. All you are saying is that you are using the same height as the real snow samples and the same peak acceleration.	This section was completely removed.
49	4544: This should go in section 4.4	Replaced as advised.
50	4545: I think there is a square missing below the root.	Yes, right! Corrected.
51	4546: ~~For realistic values of Young's modulus assumed in the model, FEM results support the argument of Sect. 3.2 saying that the block is a stiff oscillator.~~	Removed.
52	4546: ~~(i),(ii)~~	Removed.
53	4546: will have an increase of normal stress	Changed to 'will experience an increase in normal stress'
54	4546: signs of normal pressure ~~flip.~~	Changed to 'reverse'.
55	4547: ~~By definition in our model the failure is the first instant when the interface experience stresses which none of its nodes is able to sustain.~~	Removed as indicated.
56	4547: is also indicated ~~at~~ Fig. 7.	Corrected to "in".
57	4547~~:~~ ~~The behavior of this difference and the process of reducing it is discussed below for all experiments.~~	Removed.
58	4547: parameters ~~(see Sect. 4.4 and the figure's legend)~~,	Removed as advised.

59	4547: You should really show a relative difference, because the experimental time to failure varies a lot. A time difference of 2.5 seconds can be anything from about 15% to more than 100% error.	Corrected.
60	4547: earlier ~~than the observed one (Fig. 8)~~.	Removed as indicated.
61	4548: It is not clear to me what you are trying to achieve with these subsets. Rewrite for clarity.	We re-wrote this part. Also since this was explicitly introduced and explained earlier (in Section 4.2), we added an additional reference.
62	4548: ~~A more detailed interpretation of the signifi-cance of this region in terms of the Mohr–Coulomb failure envelope will follow in the discussion Sect. 6, together with comparison to other studies.~~	Removed as advised.
63	4549: ~~Some further discussion of the obtained CFEM profiles along (with c = constant) will follow in the subsequent Sect. 6.~~	Removed.
64	4551: You can probably shorten the discussion a bit	Significantly shortened.
65	4551: ~~The previous section (5)~~ has shown that even with a simple set of model assumptions, it ~~could be~~ possible to	Corrected as suggested.
66	4551: Given the number of assumptions in the model, I would not jump to far reaching conclusions. Just because you see some similarities between the numerical result and the experimental data does not mean your model is correct.	The corresponding text was completely removed.
67	4551: this should go in the results. No new results should be presented in the discussion section.	These are not new results and only further interpretation of previously introduced facts with comparison to data published elsewhere.
68	4552: again, this should be in results	Similarly, please note that this is additional discussion of main results and sensitivity tests.
69	4553: (e.g. Fig. 8~~; tests: 23, 26, 30, 39, 40~~).	Removed.
70	I would remove this last paragraph.	This paragraph was deleted.
71	The symbols for te and tm should be larger	Enlarged.
72	use different colors and a legend to make this figure more readable	Done. However, blue markers (x and o) are left as before since they share the same PHI value.

73	font is too small	Enlarged.
74	check axis labels	Corrected to Greek letters.
75	why use a log-log scale for an exponential trend. Just use a normal scale for density and a log scale on the y-axis	This was made in order to illustrate that the trend reminds a power dependency as well (as it was discussed in the main text). However, this illustration was completely removed for reducing the size of the manuscript.
76	rewrite the caption	It was rewritten.
#	**Comments from the Reviewer #3**	**Authors' response and changes in the manuscript**
	General comments	-
1	The authors have obtained the optimized values of the cohesion and friction angle of the Mohr-Coulomb failure criterion through complex procedure using the experimental data. The both parameters would be useful to describe the failure mechanism of weak layers under dynamic stresses.	-
	Specific comments	-
2	section 4532 line 19 The authors show us the density of the weak layer, and of the upper snow block in section 4541, line 8, but don't show the density of the bottom snow block. I think that for "sandwich" snow sam-ples both densities of upper and bottom are important to estimate stiffness pa-rameters of the weak layer within the both boundaries.	We added this information about density into the corresponding text.
3	section 4532 lines 24-26 Sintering time of snow particles affects the mechanical properties of snow especially for artificial snow blocks, so some readers may want know how long the samples were kept in a cold room after complete sandwich samples.	We added this information into text.
4	section 4532 line 29 For inclined samples, there are two ways to shake the samples i.e. parallel or perpendicular against the inclined geometry. Some readers may want to know the shaking direction.	Oscillations were always horizontal, without any respect to inclination. This is explicitly described in Section 3.1.
5	section 4542 line 12 Ooizumi and Huzioka (1982)'s measurements were not "high strain-rate", the strain-rates were 10ˆ-8 - 10ˆ-7 sˆ-1.	We replaced this part by another reference to high-strain FEM simulations.
	Technical corrections	-
6	section 4531 line 1 The sentence of "(Podolskiy et al., 2010b)" may be corrected to "Podolskiy et al. (2010b)".	Corrected.

7	section 4531 line 9 I am not sure, but the sentence of “The paper take into account” may be corrected to “The paper takes into account”.	Corrected as suggested.
8	section 4540 line 20 mf and A are identified here, but no a(t) used in eq. (7).	We added this missing information.
9	section 4567 Figure 1 It will be clear if the inclined angle, and shaking direction are shown in (a).	We included these details into Figure 1.
10	section 4571 Figure 5 In the text, two points of lower and upper may also refer to left and right respectively as shown in the text of Figure 6. It will be clear if three points including middle indicate the locations in Figure 4.	In order to clarify this point, we changed (in the legend of Fig. 6) upper/lower to right/left. Otherwise Fig. 4 becomes too ‘heavy’ with contents.
11	section 4575 Figure 9 In the figure the middle column’s test number may be corrected to 15 instead of 17.	Figure label typo was corrected.
12	section 4577 Figure 11 In the text “red curves” may be corrected to “red lines” as mentioned in section 4551 line 20.	Corrected as suggested.

Once again we thank the Referees for their time and efforts dedicated to improvement of our submission.

On behalf of all authors,
Evgeny A. Podolskiy