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Earth Surface Dynamics Discussions

Interactive comment on "Deep-seated gravitational slope deformation scaling on Mars and Earth: same fate for different initial conditions and structural evolutions" *by* Olga Kromuszczyńska et al.

Anonymous Referee #2

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GENERAL COMMENTS

The paper proposes a comparative morpho-structural study, investigating possible scaling relationships between parameters of DSGSD on Mars and on Earth.

Comparative and scaling approaches are useful tools that can help constraining martian processes, as well as providing environmental constraints not necessarily known for Earth. Therefore, this work has the potential to offer a valid contribution to better understand landscape evolution of Valles Marineris. However, in my opinion, some is-

sues need to be resolved before publication can be recommended. I recognize 3 major issues regarding (1) the assumptions, (2) the lack of a robust validation of the results, and (3) a dubious interpretation of some results. These are discussed in the Specific Comments and (#) more comments are added.

SPECIFIC COMMENTS

(1) (page 4 line 32) "DSGSD in Valles Marineris and the Tatra Mountains are probably not active anymore" but neither reference nor evidence is provided, therefore sounding as a personal opinion. This is a crucial point since discussions on finite strain and its distribution, from which the conclusions are drawn, are based on this assumption (page 16 lines 29-30 "The ridge aspect ratio R [..] informs on whether DSGSD, which is thought to have stopped on all the ridges, attained a similar final state.").

(2) The authors consider the ridge aspect ratio R as evidence of the slope's state of maturity (as read earlier at page 16 lines 29-30 and also at page 18 lines 1-3 "It may be inferred that on the one hand, the maturity or immaturity (instability) of ridges affected by DSGSD may be inferred from their aspect ratio."). No mention is made about previous studies, neither terrestrial nor martian, that adopt R as a proxy for slope's maturity in DSGSD. If this work is actually first of its kind, a robust validation of the results needs to be made. Otherwise, citations are needed. The authors present 6 cases of inactive DSGSD considered of post-glacial origin, 3 martian and 3 terrestrial. In order to accept the aspect ratio R as evidence of slope maturity, more cases have to be presented. In fact, also the authors mention at Page 18 lines 5-8: "Individual fault displacements across DSGSD scarps in the Tatra Mountains are similar to fault displacement in most DSGSD sites on Earth [. . .], which suggests that this conclusion may be extrapolated to other regions. Nevertheless, similar analyses need to be conducted in other ridges affected by DSGSD, both inactive and active, before general conclusions can be drawn.". I would also include cases of terrestrial DSGSD of no-post-glacial origin. This could have two implications: a) if height-to-width ratios for specific categories are found, then the height-to-width ratio for DSGSD will gain meaning with interesting implications for

martian instances; 2) if height-to-width ratios do not match specific categories of DSGSD, then this scaling relationship cannot be used as diagnostic of maturity of ridges affected by DSGSD.

(3) The third major concern arises around the two possible interpretations of the martian Site 2 and the way the results are interpreted based on these two options: (page 16 line 31) "The range of R is narrow (0.18 – 0.29) for Earth and Mars if the glacial valley at Site 2 is fully erosional [..], as interpreted by Gourronc et al. (2014). R values are much more scattered (0.08 – 0.29) and atypical compared with the two other Martian sites if the central valley in Site 2 is of DSGSD origin only [..]. Because of data coherence, we find the interpretation that the glacial valley is of fully erosional origin more likely.". In my opinion, the word atypical already shows an incorrect bias towards one interpretation rather than the other. Atypical means not representative of a type. However, as I said in point 2, this study is not statistically robust enough to actually define a result atypical. I also find the expression "Because of data coherence" and the interpretation that follows not acceptable. This looks to me selecting certain results while excluding others depending on the assumption they please the most. No results should be discarded on the basis of assumptions. Link to this is the issue that I recognize with the second assumption "DSGSD in Valles Marineris is due to post-glacial stress release on slopes". In my opinion, this is an unnecessary assumption, which rather risks to appear as a bias in the conclusions presented by the authors. Rather, the results should provide further support for the post-glacial origin interpretation. It is clear that the authors favour the interpretation of these landforms given by Mege & Bourgeois (2011) and Gourronc (2014), whose observations I find intriguing, but this cannot sound as a bias. I do not think this work shows a balanced review of the alternative explanations for the landforms observed in Valles Marineris, as instead done by Mege & Bourgeois (2011) and Gourronc (2014) in their introductory paragraphs.

A value of ~ 0.24 is given as representative of all the instances of DSGSD. However, it is never said how this value is obtained and, only by going through Table 1, I understood

that ~ 0.24 is an average value. I think providing the range of values within which the cases are found is more appropriate. Also, this links to the discussion in point 2 of the major issues.

In the abstract "On both planets, strain is distributed over the same number ($\sim$5) of major scarps;.." – If the number of the major faults is the same, it cannot be $\sim$5.

In the abstract "The measured finite strain of the Valles Marineris ridges is 3 times larger than in the Tatra Mountains,.." – This ratio is mentioned only at Page 15 line 4 "Dh for the Valles Marineris sites (-0.006) is roughly three times the values for the Tatras sites (-0.002) only.". However, in 2.2 Methods, Dh is called "scaled horizontal displacement" and nowhere else "finite strain" is used. I think more attention should be paid on clearly specify the name of the parameters, so to make it easier to follow the calculations and discussions.

The introduction does not sound well structured. A clear general introduction and description of DSGSD, which is the subject of this work, is missing. I would then discuss the evidence of such landforms on Mars. I would mention the importance of comparative planetology studies and scaling approaches, because this is what the work is about. I would conclude by clearly stating the aims of the study, which I struggle to find; explain why you are doing it.

Sub-paragraph 1.3. I do not think the authors manage to properly convey the message. First of all, I would refer to volume rather than using expressions such as "Landslides that are small with respect to mountain size" or "For landslides that involve a large fraction of the mountain slope". The effect of volume on landslide mobility is indeed not fully understood and long runout landslides, which the authors clearly refer to, are characterized by volumes bigger than 1 million cubic meters. Important examples of terrestrial long runout landslides other than the Socompa rock avalanche exist, so I would extend the citations (e.g., Saidmarreh l., Blackhawk l., Heart Mountain l., Turtle Mountain l.). To present, many papers have try to contribute to the understanding

of the influence of different parameters (volume, slope, gravity, fluid content, and so on) on landslide mobility. Just to name a few, McEwen (1989); Soukhovitskaya and Manga (2006); Lucas and Mangeney (2007); Lucas et al (2011); Johnson and Campbell (2017). Certainly this discussion is far from being close. For this reason I think that the sentence at Page 3 lines 29-32 (“This dependency of landslide propagation on slope, and indirectly on volume (and friction), initially identified on laboratory experiments (Farin et al., 2014), could be adequately documented by natural examples thanks to some very large Martian landslides (Borykov et al., submitted), much larger than any terrestrial landslide, which help populate the landslide dataset for voluminous landslides that propagate on nearly flat surfaces.”), does not pay the right tribute to the papers that have already been published on long runout landslides and does not correctly report about the current knowledge on the influence of different parameters.

There is no mention on whether authors made the DEMs or not. If they did, I would mention what software they used (e.g., SocetSet or Ames Stereo Pipeline).

Page 3 lines 2-5 “Uphill-facing normal faulting and crestal extensional deformation is indeed well documented on Earth in areas of DSGSD (review in Mège and Bourgeois, 2011). In most described terrestrial instances, such as in the Alps of Europe, Japan and New Zealand, the Andes and others, where DSGSD has been described in mountain ridges glaciated during the Quaternary (review in Mège and Bourgeois, 2011),..” – the use of “others” is not appropriate. Moreover, Mège and Bourgeois (2011) is not a review paper. More relevant papers about terrestrial cases can be cited.

Page 3 lines 8-9 “..and additionally provides a good framework to understand the detected mineralogical occurrences as from CRISM (Mège and Bourgeois, 2011; Cull et al., 2014).” – Mège and Bourgeois (2011) do not work with CRISM data but they mention the identification of sulphates and hydrated silica covering the floor of Valles Marineris providing references. Even more recently, Watkins et al. (2015) provide evidence of hydrated silicate using CRISM data.

Page 5 lines 6-7 “The observations of the Valles Marineris trough system reported here were done using Mars Reconnaissance Orbiter/CTX imagery as a baseline (Fig. 2).” – I would put a reference to Figure 1a and I would move it soon after Valles Marineris, removing “(Fig.2)”.

Page 6 lines 2-3 “DEMs generated from CTX stereo pairs have the appropriate vertical precision of ca. 10 m,..” – Vertical precision of CTX stereo-derived DEMs is not so straightforward to assess. It is usually reported as several meter. Also, in Fig. 2, a different value is reported (? 15 m). Commonly in the literature, pixel resolution is given for CTX-DEM ($\sim$20 m).

Page 6 line 17 and Page 7 lines 1-2 “the faults cutting the profiles are located and marked as two lines illustrating two mean fault dip angles, α=60° and α=70° (Fig. 3), representative of unrotated shallow normal faults in extensional settings on Earth (e.g., Gudmundsson, 1992; Acocella et al., 2003). – I could not find any reference to fault dip angles in Acocella et al. (2003). In Gudmundsson (1992) I have found “Fault dip ranges from 42° to 89°, with a mean of 73°, but nearly 80% of the fault dip between 65° and 79°”. I am not saying that the range 60° and 70° is not reasonable, I just do not see the justification in the literature proposed. Also, the authors clearly show that they are in favour of a DSGSD with a post-glacial origin scenario, almost discarding alternative explanations (Page 2 lines 20-22 “They denote extensional tectonics, but boundary forces that result in crustal “rifting” are unlikely to be the cause of this deformation even though rifting is frequently considered to have been a major contributor to the formation of some of the main Valles Marineris chasmata”). For this reason, I find slightly incoherent the reference to Gudmundsson (1992) and Acocella et al. (2003), who worked in the rift zone of Iceland and on flank instability of Mount Etna, respectively. Is there any fault dip range given for DSGSD in the literature?

Page 7 line 1 “..and marked as two lines illustrating two mean fault dip angles, α=60° and α=70° (Fig. 3)..” – Figure 3 show just one line representing a general fault. So, I would remove the reference to Figure 3 at this line.

Page 7 lines 6-7 “The 10° angle interval is considered as the error on the true angular value, which cannot be retrieved from topography..” – I would place this sentence near to when fault dip range is discussed. This is the explanation to why the range 60°-70° is considered.

Page 8 line 9 “Three sites [..] were selected in Valles Marineris, based on DEM generation possibility,..” – Without DEM the study cannot be conducted, so not necessary.

Page 10 lines 5-9 “Site 5 [..] is a ridge west of VeÄ¿ká Garajova Kopa on the way to VeÄ¿ká Kopa. [..] The 900 m wide ridge rises up to 250 m above the adjacent valleys. The height to width ratio R is 0.28.” – These values correspond to Site 4 in Table 1 (??)

Page 11 line 3 “topographic profiles were measured” –Profiles are traced. See also caption Fig.4.

Page 15 line 4 “Dh for the Valles Marineris sites (∼0.006) is roughly three times the values for the Tatras sites (∼0.002) only.” – I did the calculation for Dh and I cannot obtain the same values. For terrestrial sites (4, 5, 6) I get ∼ 0.003, whereas for martian sites I get ∼0.008 if sites 1, 2b, 3 are taken; ∼0.008 if sites 1, 2a, 2b, 3 are taken; ∼0.007 if sites 1, 2a, 3 are taken; ∼0.006 if sites 1, 3 are taken. Please, explain how you did the calculation.

Page 16 lines 2-3 “This is interpreted as a consequence of removal of the highest part of the ridge, as discussed in Section 5.” – I cannot find the discussion in Section 5.

Page 16 lines 11-12 “..the number of DSGSD faults in Valles Marineris (Tables 2 to 4) is not significantly different from the number of such faults in the Tatra Mountains.” – No count of faults is given in any table. Also, there is not a Table 4.

Page 16 line 13 “The very large fault offsets measured on individual faults in Valles Marineris require cumulated events (e.g., Fossen, 2010).” – Fossen (2010) is a textbook. If this has to be the reference, please provide the chapter and the page where

the information can be found.

COMMENTS ON FIGURES:

Figure 2 – I think this is a superfluous figure.

Figure 3 – I would add a sketch/model showing which are the reference points from where the height and the width of a ridge are measured. For example, this information is always present in papers on long runout landslides, making clear that height drop and length of a landslide are measured from the highest point of the scarp to the furthest point of the deposit.

Figures 4, 5, 6, 7 – I would create a border/space between the images.

Figure 6 – I think in 6b and 6c codes of the area (“mc” and “co”, respectively), then used in the name of the profiles in Fig.8a, are missing. Also, it took me a bit to find the codes “c1” and “c2” in 6a. I would create a box around the profiles.

Figure 8a, 8b – I think unit of measure should be placed on the graphs rather than in the caption.

COMMENTS ON TABLES:

I would not use progressive numbers for site IDs, rather some other way that would help to identify immediately on the graphs which case is from which planet.

Table 2 – I suggest using the symbol x bar and z bar instead of writing “Mean horizontal fault displacement” and “Mean vertical displacement”, respectively.

Table 2 – I suggest it to have the same style of Table 3. In Table 2, α is placed under Site ID, which does not make sense.

I would group Table 2 and 3 (and maybe also 1).

Values of Dh and Dv are not given in any table.

COMMENTS ON SUPPLEMENTARY MATERIAL:

I think that the data that the graphs are trying to show should be better made available in a table. On the graphs, the values of the displacement is not clear, making it difficult to follow the calculations that have brought to the results reported in the manuscript.

I would add all the profiles obtained for this study, rather than just having few examples in the manuscript.

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