

Interactive comment on “New insights on the predisposing factors and geomorphic response to the largest landslide on emerged Earth surface: the Seymareh rock slide - debris avalanche (Zagros Mts., Iran)” by Michele Delchiaro et al.

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The study investigates valley-bottom evolution of the Seymareh River valley bracketing the gigantic, prehistoric Seymareh landslide. It contributes new details on drainage variability, particularly changes in response to the formation and later erosion of the landslide dam, supported by new geochronologic controls. The manuscript does not, however, provide new insight on causes of the landslide or its numerical modeling, despite these topics appearing repeatedly in the abstract, introduction and conclusions.

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This has the potential to be an interesting contribution to understanding of 1) the Seymareh landslide and 2) landscape evolution of the simply folded zone of the Zagros fold-thrust belt. These are very welcome additions as both subjects have received less attention than they deserve. The authors' focus on long-term valley-bottom geomorphic change is particularly interesting as such an approach has received minimal attention in past examinations of this landslide.

The main contribution of the paper – valley-bottom geomorphic evolution – fits with the journal's focus and is as far as I know unique to any study conducted in Persia.

The manuscript unfortunately suffers from numerous deficiencies that prevent me from being able to recommend it for acceptance in its current state. It could be reconsidered following very substantial reworking, additions, and improvements. The greatest issues include:

- i. Overall imbalance of content, with over half of the manuscript comprising background material;
- ii. Unnecessary details and focus on some topics (numerical modeling, pre-failure creep, seismicity and hazard/risk) that take up large parts of the abstract and/or introduction, but that are not part of the current contribution and are not directly relevant to its conclusions;
- iii. Incomplete review of previous work on the Seymhareh landslide and geology of Kabir Kuh, including misattribution of several findings and interpretations;
- iv. Insufficient methodologic descriptions that prevent the new work from being properly evaluated or replicated;
- v. Confusing organization that includes: a section purported to present new material (section 4) comprising largely a repeat of what was already known; a results section (section 5.2) containing a mix of observations/interpretations and results; and a discussion (section 6) presenting apparently new observations/interpretations.

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vi. Writing that is commonly overly wordy, confusing, or incorrect.

The major issues, including various aspects of the six points above, are listed below. These are followed by minor issues, which are predominately examples of language and writing issues. The list of minor issues is not comprehensive, but provides an idea of the types of problems that should be addressed throughout the manuscript.

MAJOR ISSUES

1. The writing is very wordy and convoluted. Many sentences are unnecessarily long and complex, making them hard to follow. Grammatical and language errors are abundant. The scale of these problems make substantial rewriting necessary, and thus in my view is a major issue. Many examples of such instances are provided in the list of minor issues below, but this is not an exhaustive list.

2. The title does not accurately convey the main thrust of the paper. It suggests that the paper provides new understanding of the causes of the landslide, which it does not. This paper is about valley-bottom geomorphic evolution before and after a gigantic landslide.

3. The abstract does not summarize the present study well, and instead mentions all sorts of things that are not part of the authors' work; although some of these are peripherally related (hazard, seismic triggering, causes of the landslide) they do not constitute anything new as far as I can tell from the presentation of the rest of the paper. At the same time, the abstract lacks details about some of the major interpretations and results from the body of the paper. It needs to be rewritten and streamlined.

4. The summary of previous work on the Seymareh landslide is missing many key points and attributes some details to the wrong sources. Other details are not attributed at all. For instance:

a. Page 3, line 6: Some important and very relevant contributions of Roberts (2008) and Roberts and Evans (2013) are not mentioned. Those sources propose a detailed

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model of how the geologic and tectonic evolution of Kabir Kuh predisposed the slope to such large-scale failure, including formation of structural/kinematic and rheological control. As far as I can see, this contribution is not recognized in the current paper, despite it being directly related to the authors' claimed contribution of improved understanding of factors predisposing the slope to gigantic failure.

b. Page 3, line 9: It is not sufficiently clear that the age estimate of 9800 radiocarbon years is based on the interpretation of three separate radiocarbon ages. This 9800 a BP age is taken from Roberts and Evans (2013) and must be cited accordingly. The ages provided by other sources - at least those from Griffiths et al. (2001), which were not influenced by the 'hard-water effect' – should also be noted so that the reader does not have to refer back to Roberts and Evans (2013).

c. Page 3, line 6: Yamani et al. (2012) provide no new details on emplacement mechanisms of the landslides (at least not from the details in the English language extended abstract of their paper written in Farsi). If there are some details missing from the Yamani et al.'s English text that their present manuscript refers to, it would be very helpful to provide translated quotes in the supplemental material. Otherwise, mention of new details on landslide emplacement attributed to that source needs to be removed. Yamani et al.'s (2012) main contribution comprises some general details on the evolution of lake drainage.

5. The paper includes a lot of largely unimportant, or at least overly specific, background details. Many of the details about tectonic features and some of the details about seismicity in section 2 are well beyond what is necessary to provide relevant background to the reader. These extraneous details could appear in the supplement to provide further context for the interested reader, but they take up too much of the main paper. Given that modeling is not part of the present paper, much of the background provided about modeling is irrelevant.

6. If the mechanisms and behaviour of large bedrock landslides are to be discussed,

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some recent reviews pertaining to this topic should be cited (principally Brideau and Roberts [2015], which includes this event as a case study, and Hermanns and Longva [2012]). Those sources provide numerous additional references on progressive failure that should also be considered if the authors can make a suitable case for discussing this topic.

7. Much of the background information lacks proper referencing, including tectonic setting (section 2), seismicity (section 2), and geologic setting (section 3). For example, the paragraph starting on page 4, line 27 presents many details that are clearly not part of the current study, but only provides references in two places. Furthermore, this generalization of sources does not allow the reader to sort out what details have come from what sources.

The hypothesis cited in the final paragraph of section 2 is based on much more than just the frequency of strong earthquakes in the region, as this submission suggests. Progressive steepening of the slope at a very slow rate relative to the modern recurrence rate of nearby strongly felt earthquakes is a crucial consideration as it makes failure initiation in the absence of seismic loading hard to explain.

8. Thickening of the Pabdeh Formation (top of page 6) very well may have in influence on landscape development. However, the position of the northwest-trending deformation front also strongly infers the westward change in landscapes. This was also recognized by Oberlander in his work, but appears to have been overlooked.

9. In contrast to what the authors claim, I see no evidence that this work has revised the stratigraphy of the study area to any substantial degree. The stratigraphy of the part of this Zagros fold-thrust belt is extensively reported in previous work (much of which as not been referenced here). The stratigraphy described here matches very closely (in sequence order, composition, and thickness) with the stratigraphy already reported in the literature. For instance, I see very little difference between the sequence in Fig. 5 of the present manuscript and the stratigraphy summarized from review of existing

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literature by Roberts and Evans (2013, their Fig. 5).

Several important references on the sequence are missing and should be included. For the overall region this includes James and Whynd (1965) and Alavi (2004). Detailed mapping of Kabir Kub conducted by Iran Oil Operating Companies (Setudehnia and Perry, 1967; Takin et al., 1970; Macleod, 1970) already covers much of what the authors would have covered in their 'new' mapping presented in section 4 and in Fig. 5.

The authors need to first clearly describe what has already been documented (in a section on geologic background). Only after that should they try to justify how their 'new' stratigraphy differs. From what I can tell, they contribute only some additional detail on the Pabdeh Formation, although at least some of this is similar to that reported in the Iran Oil Operating Companies maps. A much more convincing argument will need to be made if any new contribution to the area's stratigraphy/geology is to be claimed.

The details presented in section 4 nearly all belong in the background material. This further highlights an issue with the paper's layout and balance: the new contribution of the 15-page manuscript doesn't start until page 9, meaning that over half of the paper is background information.

10. The Gachsaran Formation has a high gypsum content, which dominates its geomechanic and geomorphic behaviour. The description provided here (Page 8, line 17) instead suggests that it comprises only more typical clastics. Given this manuscript's focus on valley-bottom evolution, this unit needs to be accurately characterized.

11. The details, or even relevance, of anticline flank dips (second paragraph of section 4) is unclear. Dips along the three sections probably need to be considered more carefully. Due the nature of the anticline, structural variation is to be expected. Dips will of course decrease to the southeast toward the nose of the anticline. What is possibly more interesting is how much steeper dips are to the northwest beyond the

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mapping presented here. Furthermore, this is a complex box fold with a hinge near the upper part of the Seymareh detachment zone. Downslope variation flank dips will reflect this structure.

Due to the complexity of the fold, stratigraphic position will also affect dip. For example, Roberts and Evan (2013) noted steepening (see their Fig. 9H for example) of the Eman Hassan member in the upper, central part of the landslide scar. In contrast, the adjacent upper surface of the Asmari Formation on either side of the landslide is much less steep. Variations between other units are noted also in mapping by Iran Oil Operating Companies (summarized in part in Fig. 4B of Roberts and Evans). Such variation has important implications for dip-slope failure of the flank.

In light of these points, it would thus be very helpful (and far more informative) if the authors provided a detailed map of their structural measurements, and clarify how these may build upon those provided by past studies of the landslide and by mapping by Iran Oil Operating Companies and by Roberts and Evans (2013). Conversely, if these measurements are based only DEM profiles, that needs to be clearly stated. In any case, how well profiles through the eroded core of the anticline represent the Asmari limestone limbs beneath the valley floor needs to be evaluated given stratigraphic variation in dips relating to the complex nature of the fold.

Finally, profile C-C' is oblique to the true dip of the anticline flank and appears to thus under represent dip of the Amsari surface (based on Fig. 5, it appears that the apparent dip, not the true dip, is being represented).

12. Is this the reconstruction of the Asmari carapace (Page 8, line 29) immediately pre-failure or a reconstruction of the anticline prior to unroofing? Please clarify. In the latter case, 2100 m a.s.l. is a substantial underestimate and the structure of the box fold suggests that the Asmari carapace extended much higher.

13. The authors' point about the position of Seymareh River (Page 8 line 31) does not become clearly relevant later one in the paper. Depending on its significance for

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the current study, which remains unclear, the authors will need to clarify whether this river position is related to migration of the channel within the Lake Seymareh lacustrine deposits (i.e. following valley damming and sediment in-filling of the Lake Seymareh basin) or is the result of some older physiographic control. The river has obviously migrated to the northwest in the last several hundred years as a Sasanian-era bridge over the old river position at Pul-i-Shikari is nowhere near the modern river (see observation and discussion by Harrison and Falcon, 1938).

14. I cannot understand what the final five lines of section 4 mean. I assumed the kinematic release suggested here is that of the Seymareh landslide. What is the 'connectivity' supposed to be? How are the flatirons envisioned to control sliding? Note that several previous studies suggest fluvial undercutting of the slope as the source of kinematic freedom at the slope toe. However, breakout across the upper units is also necessary for the failure to have occurred as the failure surface cuts stratigraphically upward in the downslope direction (Roberts and Evans, 2013); this feature is an important part of the kinematic release.

15. The methods section lacks sufficient detail. What are the source and scale of the air photos used? Were they interpreted quantitatively or only qualitatively? What specific imagery was used from Google Earth (there is of course a very wide range of imagery types and qualities available in that software)? What as the source of the map used? What inputs specifically were used the creating the DEM (current wording is unclear)? No methods are provided for the new geologic investigation that the authors claim to have conducted.

OSL can be a finicky technique. Many critical aspects of the sampling are not considered, particularly those necessary to rule out partial bleaching during sample collection and transport: was the slope cleared off first? was an opaque sample vessel used? how far was it inserted into the slope? OSL sampling methodologies vary quite a bit, so unless the approach exactly follows that of a pervious study, simply citing in past source here is insufficient.

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16. The results section (section 5.2) is rather hard to follow and contains a mix of observations, interpretations and results. Furthermore, several observations/interpretations (conveyed in Figs. 14 and 15) are skipped over here and are referenced only in the discussion. The formatting of this section needs work. I see no benefit to using lists here (or anywhere in the paper); all text should be in paragraph format. Several parts of the lists are not even full sentences.

Issues with in the content include a lack of clarity over how the authors believe the dated sequences to relate to each other. For example, the wording at the start of the section (Page 10, line 9) seems to suggest that the lacustrine terrace pre-dates the landslide, but I cannot imagine how this is possible. Such as suggestion also conflicts with the post-landslide age reported for samples SEY4 and SEY5.

17. The discussion lacks any details about reliability of the OSL ages presented here. There are many possible error sources in this technique, but it is unclear if these were considered.

18. The discussion suggests that the current study provides some new insight on the geologic succession and its role in mass rock creek (MRC). I see no such contribution in the paper. The authors seeming claim in the abstract and introduction that their study somehow addresses pre-failure creep, but this topic is not investigated in any detail.

Pre-failure creep is hardly mentioned, and even then is based only a couple of field observations. The suggestion that features noted in the Pabdeh Formation (Page 13, line 2) indicate pre-failure creep is not sufficiently supported. It is also possible that these features are not a result of progressive failure of the slope. The plastic deformation shown in Fig. 14B could well be the result of pre-failure creep, but not enough detail is provided to evaluate this. Rock mass strength reduction and associated deformation is also to be expected as a result of fold formation (see Roberts and Evans, 2013 and references therein). The brittle deformation shown in Fig. 14C could well be the results of sliding during catastrophic failure, so I see no reason to use it to argue for pre-failure

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creep.

19. The authors also bring up numerical modeling in the abstract, introduction and discussion, despite it not factoring into the study. Perhaps they mean to indicate that their results could be used to guide/inform numerical modeling, but their wording is unclear. Given that numerical modeling is not addressed in the study, the background on modeling in the introduction is too extensive and Figure 14 seems irrelevant.

20. From what I can tell, the authors provide no clear new evidence regarding predisposition of the slope to failure. They start to suggest two interesting aspects, but do not adequately address them. The first aspect is that of pre-failure creep (noted above). The second, which is more closely tied to their investigation, is the possibility of knickpoint migration along Seymareh River, which is inadequately communicated. The extensive evaluation geologic and geomorphic controls on the failure by previous studies (particularly Roberts and Evans, 2013) is hardly addressed, although authors of the present study claim that this is one of their main focuses.

21. Why are landslide kinematics described in the discussion (top of page 13) when they are not part of the new work presented here and when the detailed examination of this topics by previous workers is not mentioned in the background sections? For instance, the geomechanical strength contrast between the Asmari and Pabdeh formations is mentioned in the current paper, but has not been characterized. It is, however, approximated in Roberts and Evans (2013).

What evidence is there for kinematic freedom provided by gullies along the flank of Kabir Kuh (Page 13, line 13)? The lateral margins of the main landslide are nearly vertical features following a major joint set in the Asmari Formation (characterized in Roberts and Evans, 2013 and references therein, but not mentioned here). There is no evidence I can see for these being related to fluvial processes. Roberts and Evans (2013) propose that these features are instead inherited from the tectonic history of the Zagros' simply folded zone.

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22. What is the basis for the suggestion that failure was preceded by an 'elapsing time' on the order of 100 ka (Page 13, line 17)? It's not even clear what this period is meant to represent. Is this a period of pre-failure creep? The period between the knickpoint passing the toe and the slope failure? This is very hard to follow.

23. The conclusion has several issues. It seems to include material – some kind of modeling related to the Seymareh River valley – that is not only not part of the present study, but as far as I can tell is not included in any other published research. I do not see how this fits in, other than also being mentioned in the abstract and introduction.

The list summarizing landscape evolution of this part of the Seymareh River valley seems very similar to the list presented on the previous page in the in the discussion, and is thus quite redundant.

The conclusion ends with a sentence about seismic triggering, which is hardly mentioned in the paper other than stating that pervious workers have suggested it (and even then fails to adequately explain what has been done before). I don't see how the new work done here will contribute to evaluation of a seismic trigger. This text seems to be irrelevant to the conclusions of the paper.

24. The figures are too numerous. Several can be combined (particularly the photos) and others seem to have limited relevance. Fig. 2 is probably more appropriate in the supplement given the lake of relevance of seismicity to the current study. Figs. 1 and 3 could probably be combined, especially if some of the extraneous detail in Fig. 1 is removed. Figures 13 and 14 are irrelevant to the main focus of the paper. Fig. 13 does not add anything to the paper. Figure 14 relates to the suggestion of pre-failure creep of the slope, which contrary to what the authors state in the introduction of the paper, is not a major component of the present study. Figure 15 seems to be an afterthought, although it has far more relevance to the paper's focus on valley-bottom geomorphology than either of the preceding figures.

The list below provides references for several works that the authors should consult

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and that are missing from the current paper:

Alavi, 2004. Regional stratigraphy of the Zagros fold–thrust belt of Iran and its profore-land evolution. *American Journal of Science*, 304, 1–20.

Brideau and Roberts, 2015. Mass movements in bedrock, in: *Landslides Hazards, Risks and Disasters*, [Davies and Shroder, eds.]. Academic Press, Amsterdam, Netherlands, 43–90.

Griffiths et al. 2001. Environmental change in southwestern Iran: the Holocene ostracod fauna of Lake Mirabad. *Holocene*, 11, 757–764.

Hermanns and Longva, 2012. Rapid rock-slope failures, in: *Landslides: Types, Mechanisms and Modeling* [Clague and Stead, eds.]. Cambridge Univ. Press, Cambridge, UK, 59–70.

James and Wynd, 1965. Stratigraphic nomenclature of Iranian Oil Consortium Agreement Area. *AAPG Bulletin*, 49, 2182–2245.

Macleod, 1970. Kabir Kuh, 1:100000 Geological Map. Iran Oil Operating Companies, Geological Exploration Division, Tehran.

Setudehnia and Perry, 1967. Dal Parri. 1:100000 Geological Map. Iran Oil Operating Companies, Geological Exploration Division, Tehran.

Takin, M., Akbari, Y. & Macleod, J.H. 1970. Pul-E Dukhtar. 1:100000 Geological Map. Iran Oil Operating Companies, Geological Exploration Division, Tehran.

MINOR ISSUES

1. Page 1, line 10: The anticline is variously referred to as ‘the Kabir-kuh fold’, ‘the Kabir-kuh Fold’, ‘Kabir-kuh fold’, and ‘the Kabir-kuh’. Kabir-kuh is a proper physiographic feature whereas the fold feature is not officially recognized as a name. Thus, the only proper version of the naming used here are ‘Kabir-kuh’ and ‘the Kabir-kuh fold’.

2. Page 1, line 15: Proper capitalization and use of 'the' are: 'the Seymareh River valley' and 'Seymareh River'. This needs to be corrected throughout the manuscript.
3. Page 2, line 28: What is meant by 'different evolutionary stages'?
4. Page 2, line 28: 'allows to construct' is improper language.
5. Page 2, line 28: What are 'interesting valley sections'?
6. Page 2, line 33: The location description is incomplete. The Seymareh River valley straddles the border between Lorestan and Ilam provinces, respectively to the east and west of the river. The landslide initiated in what is now Ilam province, but most of the debris lies in Lorestan.
7. Page 2, line 34: What is meant by 'evolutionary scenarios'? 8. 9. Page 3, line 1: I am not familiar with the region 'External Zagros Mountains'. Is this the simply-folded zone/belt?
10. Page 3, line 13: 'Seimareh' should be 'Seymareh'. Although various spellings have been used over the years, 'Seymareh' seems to be the currently recognized version. In any case, spelling should be consistent throughout the manuscript. This crops up in a few other places.
11. Page 3, line 15: It is far more informative to state here what the study achieved, rather than what it intended to achieve. Also 'aims at better understanding' is improper English.
12. Page 3, line 16: Here, in the abstract and again later on risk (or risk mitigation) is thrown in. However, this topic is not explored. Practically, the only mitigation would be complete evacuation (either temporality based on some kind of warning system or permanently) of an area that could experience landslide of this magnitude and stabilization or localized avoidance are impossible. Furthermore, the very low probability of a landslide of this magnitude means that its risk is potentially rather low.

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13. Page 3, line 25: What is 'e.g.' used? Also, this is hardly the more relevant source for this statement given that several studies have investigated the landslide.
14. Page 3, line 27: Units should be m a.s.l. as this is an elevation.
15. Page 4, line 1: 'The Zagros' is not the proper way to refer to the range. This statement also requires references to back it up.
16. Page 4, line 15: 'landslide' not 'Landslide'.
17. Page 4, line 15: Given that this is a new paragraph, reference back to the previous content (using 'latter') is a confusing.
18. Page 4, line 22: Parentheses missing around publication year.
19. Page 4, line 22: What is meant by the 'onset of the [sic] deformation'? Is this supposed to be propagation of the deformation front?
20. Page 5, line 13: I've never come across 'Delful Zagros' as a term. Are the authors certain that this is a properly recognized physiographic region?
21. Page 5, line 14: Missing year of source.
22. Page 5, line 21: The mobile and competent units have not yet been introduced, and are part of the geology not the geomorphology (as the section title would suggest). These have not yet been introduced. The geology should be briefly summarized before the geomorphology, especially given the apparent influence of the former on the latter. A few of the units are mentioned in the following lines, but the geology is of course much more complex than that.
23. Page 5, line 22: This fold is in Ilam province, not Lorestan. The boarder between them in this area follows Seymareh River.
24. Page 6, line 28: Should be 'the Asmari Formation'. Spell formation out throughout the text of the paper; do not abbreviate to 'Fm.' (as on line 31).

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25. Page 6, line 5: This is general physiographic background that should appear much earlier on in the paper.
26. Page 6, line 6: Identify the lakes here and how they were formed (i.e. which rivers were blocked). Lake Balmak is not named until the discussion and is hard to place in the figures. It would also be helpful to very briefly note that much of the previous literature calls this lake Chah Javal.
27. Page 6, line 9: Regarding ‘. . . formed in response to a sequence of landslide’, clarify whether this is multiple separate landslides or all related to the Seymareh landslide.
28. Page 6, line 9: Consider simplifying to ‘the landslide dams’ so that the reader does not mistake your meaning to be multiple landslide dams of Seymareh Lake (instead of multiple lakes dammed by the Seymareh landslide).
29. Page 7, line 12: Combining ‘none’ and ‘neither’ forms a double-negative. Also, ‘study’ should be ‘studies’.
30. Page 7, line 13: Specify ‘fluvial’ geomorphic markers (and remove ‘the’).
31. Page 7, line 17: ‘landslide’ not ‘Landslide’.
32. Page 7, line 18: Presumably ‘refer to’ means ‘date to’?
33. Page 7, line 19: Geologic ages should be late/early, not upper/lower. The latter pertain the stratigraphy, not ages.
34. Page 7, line 19: The ages should be ‘Late Cretaceous’ (an officially recognized age) and ‘early Miocene’ (not an officially recognised age).
35. Page 8, line 21: Unless I’ve missed a break, the paragraph ending on this line is massive and needs to be broken up.
36. Page 8, line 29: Do not abbreviate ‘Formation’.
37. Page 8, line 34: The river name should be ‘paleo-Seymareh river’ as this is not an

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officially recognized name.

38. Page 9, line 12: 'literature' is insufficient. What were the sources?
39. Page 9, line 18: Unclear what is meant by the 'data where [sic] acquired'.
40. Page 9, line 20: Write 'minutes' out in full.
41. Page 10, line 4: Which components of the Hydrology toolbox were used?
42. Page 10, line 17: Should be 'the Seymareh River gorge'.
43. Page 11, line 10: Why is this knickpoint '...the most interesting...'? The authors seem to be implying that this may related to instability within the flank of Kabir Kuh, but the reader can only guess.
44. Page 13, line 20: Lake Balmak is mentioned here for the first time. Why?
45. Page 13, line 24: What about the drainage was progressive? The actual drainage is now well characterized here.
46. Page 13, line 28: This duration for the lake needs to be compared with estimates provided in other sources.
47. Page 13, line 30: This is the only place this figure is cited. The figure does not alone indicate what the authors suggest. Has some further work been done on this stratigraphic section that I missed?
48. Page 14, line 3: What is meant by 'time scan'?
49. Page 14, line 5: I again see no benefit to a list instead of writing out the description in proper paragraph format.
50. Text in some figures, particularly the labels (and markers) for sample locations in Figs. 7 and 10, is too small and thus hardly legible.

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