Earth **Surface**
Dynamics

Discussions

Interactive comment on "Using Google Earth Engine to monitor co-seismic landslide recovery after the 2008 Wenchuan earthquake" *by* Wentao Yang et al.

Odin Marc (Referee)

odin.marc@get.omp.eu

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The authors present a regional analysis of remotely sensed vegetation index in the Wenchuan region to assess the dynamics of landslide surface revegetation and how it depends on various environmental controls. Many studies (including several by the same authors have done similar exercise in the last 5 years (eg, Yang et al 2017, 2018a, 2018b, Yunus et al 2020), the novelty of this work is that it covers an area larger than previous studies, that it is based on Landsat imagery (with a finer resolution 30m than previous work based on Modis) and done with Google Earth Engine. The topic could be of interest to Esurf I would say and the structure and writing of the manuscript

is fairly good. However, the main problem is that beyond the technical novelty stated above the manuscript does not bring much and appears to bring very little new scientific content (Beyond the site there are only 2 figures, which basically are not very different from what can be find in Yunus et al 2020 or previous paper of the author Yang et al 2018a,b).

So it may be better redirected to a regional journal ? Or become a technical note in Esurf ? But even if it is retargeted like this it would still require substantial modification. Indeed the methodology is not always sufficiently described, and the discussion is often quite poor. I give below the major limits of the work and a series of Line by Line recommendation.

Odin Marc

Major comments:

1) The author use the term of "landslide (surface) recovery" which is inaccurate and too vague relative to their methods. Recovery can mean many things, while the author are very specifically measuring the temporal evolution of vegetation index. So I think they should replace everywhere by "Landslide (surface) revegetalisation" . Below I gave a few of the lines where this occur but the change should be through out unless the authors have sentence where they specifically discuss various option for landslide recovery (deposit armoring , or other form of greain size changes; Soil formation, bedrock/regolith properties changes ?)

2) I think the Pre-seismic EVI should not be used as a controlling factor but used to normalize the EVI increase rate. The simplest scenario of revegetalisation is that vegetation similar to the pre-seismic vegetation will recolonize the landslide areas. Thus a large pre-seismic EVI will tend to a large post seismic EVI and vice versa for small pre-seismic EVI. As a result it is intuitively expected that the absolute recovery rate is correlated with pre-seismic EVI. I think it may be more interesting to present all the result in terms of EVI/pre-seismic EVI to obtain a score from 0 to ∼100% which would

mean a full recovery. The rate of this normalized revegetalisation could be analyzed relative to all factors as done by the authors.

3) The result analysis is clearly lacking a cross-correlation analysis... Why to study Elevation and TPI if they both are very highly correlated ? Elevation is the physically meaningful one as it related to temperature and thus the vegetation type and probably growth rate ... TPI is less meaningful for a plant I suspect... Also I suspect Pre-seismic EVI and Elevation to be strongly correlated (because of temperature...), hence the importance to study EVI/EVI_pre-seismic (see comment 2). Actually Fig 4 of Yang et al 2018b is exactly showing that PreEQ-NDVI and Elevation are strongly related. So it makes no sense to treat Elevation and Pre seismic EVI as independent, and strongly supports the normalization by pre-EQ EVI.

4) The methods miss many details : How were treated pixel relative to landslide boundary (i.e. when they were partly across a slide and partly across undisturbed slope ?). What about seasonal variations ? Several previous studies (eg, Yang et al 2018a, Fig 3) do account for their treatment allowing to have a finer control on the revegetalisation. I wonder why this is not done here.

Last, the author state they have shown that Linear recovery is the best assumption, which leaves me very skeptical. Even in their own work (Yang et al 2018a, Fig 3) an example of recovery of the NDVI is shown and it is quite clear that the revegetalisation is far from being linear : there is a few years (3-4) with no NDVI trend , then an increase in NDVI which seems to accelerate. This could make sense, as there may be some delay before new plant colonize the area, and then as vegetation develop multiple species can make the rate of biomass accumulation (and NDVI increase) increase with time, giving a non linear revegetation rate. One way to make the work scientifically richer could be to specifically extract various parameters describing the shape of the revegetalisation curve (EVI or NDVI trend) for as many landslide pixel as possible... Some other aspects are poorly detailed (see Line by Line comments below).

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5) After accounting for the changes in the other major points, the discussion should be substantially improved, and I suggest the author to also compare their work and result to other studies which have looked at revegetation on landslides not necessarily only in the Wenchuan area.

Two Ex from Taiwan (other exist) Schomakers, J., Jien, S.-H., Lee, T.-Y., Huang, J.-C., Hseu, Z.-Y., Lin, Z. L., Lee, L.-C., Hein, T., Mentler, A. and Zehetner, F.: Soil and biomass carbon re-accumulation after landslide disturbances, Geomorphology, 288(Supplement C), 164–174, https://doi.org/10.1016/j.geomorph.2017.03.032, 2017. Lin, W. T., Lin, C. Y. and Chou, W. C.: Assessment of vegetation recovery and soil erosion at landslides caused by a catastrophic earthquake: a case study in Central Taiwan, Ecological engineering, 28(1), 79–89, 2006.

Line by Line comments L62 : No need to say its a cloud based: "In this work we use the GEE". ALso surface recovery is ambiguous. I would say : "to track the revegetalisation of coseismic landslide" rather than map surface recovery...

L64: "is easy to use" rather than has the advantage of easy-to-use ... L64-65: switch the subject : "allows researcher unfamiliar with remote sensing techniques to process efficiently large number of images "

L65, 89, 98 , 115 , 132, 143, 161 etc etc: replace recovery by revegetalisation.

L84: What do you mean the best processing level ? Clarify or remove.

L113, 121 : replace OSL by OLS

L114: replace "in a year-round" by seasonally

L117-119 : I do not understand why the authors estimate only the 15 of July vegetation level... The vegetation EVI (as well as solar radiation in relation to view angle/passage time) must vary in a seasonal repeatable way. So it should be possible to stack the EVI of every months preceding the EQ to obtain an annual cycle of EVI and be able to compare this to the months and year following the EQ... It would allow to have a much

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finer temporal resolution of the vegetation recovery.

L127: remove "including" it is unecessary and rather confusing.

Paragraph 2.2.3 : Should be written "Peak Ground Acceleration" everywhere.

L131 : Description of Gallen 2015 data is inaccurate and incomplete. The authors must improve that.

L134 : What contains ZHang 2007 ? Vegetation type at which resolution ? With which value ? This need to be clarified and possibly example data be shown in the supplement...

L141: I guess you mean "we randomly selected 1 million pixels" . If not please clarify.

L156 : You should say here something like " Therefore, we exclude the negative EVI changes"

L157: The "mean revegetalisation rate"

L162-165 : This is a repetition from the methods , to be removed...

L165-166 : Repetition from the result sentence to remove or change.

L166-167 : Do not understand this sentence. To rephrase.

L169 : You did not define TPI. Guess it is Topographic Position Index (to be defined/reference in the method)

165-177 : This paragraph has poor writing ... and is insufficiently quantitative : Could we have the correlation coefficient ?

Fig 3 : How was landslide depth obtained ? This should be detailed in the method section... Or depth should be removed (possibly better if it is just a rescaling of Area) Tangent curvature was in the method among the 12 and disappeared from this figure ?

L195-200 : I think all this argument about TPI are unlikely. Instead I think TPI is highly

correlated to ELevation (=Temperature) which is most likely the control.

L200-210 : Temperature role is underplayed. Correlation between Pre-seismic EVI and ELevation should also be assessed, as they very likely are correlated. The role of climate, or landslide area and slope likely cannot be assessed before the normalization by EVI_pre is not done...

L217: "The recovery of landslide surface can be used to indicate the duration of a major mountain earthquake (Yang et al., 2018a)" This sentence makes no sense. To be removed or rephrased.

L228-229 : Last sentence is a repetition of what is above. TO be removed.

Paragraph 4.4 : Rather useless in my opinion. Sure GEE stores imagery and can process lot of data for various purposes, but no need to write 15 lines about it with tons of self citation ...(Just cite Gorelick, as done before... Further the focus of past studies on specific zones is due to the fact they aimed at mapping landslides accurately, which is not what the author are doing. They use maps from other (made often with resolution higher than Landsat) and then extract pixels within them to track the evolution of their EVI.

So L 238 "to map landslides using all available Landsat" is simply a wrong statement to be removed. GLobally this paragraph should be mostly shrinked/removed.

Conclusions : "We found >99% landslide surfaces have been recovering since 2008" I would rather say "surfaces have experienced some revegetalisation as tracked by EVI increased" or something like that, less ambiguous.