Dear Editors,
Dear Authors,

Thank you for letting me read and revise this interesting manuscript.

This manuscript presents an extraordinary dataset:
Very interesting new and detailed data on the topography of the alluvial fans and the surface change of these fans over a three month period in combination with bathymetric and backscatter data of the Plansee lake floor. These data quantify the current state of these surfaces and a short term change of the terrestrial part. Secondly, the data comprise sediment cores in which the turbidity-current event stratigraphy has been analyzed over longer time scales (4000 yrs). The paper showcases exciting data, is well written and mostly well illustrated.

However, I do have a major concern regarding the logic behind this paper, that assumes that every turbidite deposited – besides the ones that are attributed to earthquakes from another publication - corresponds 1:1 to a single debris flow event onshore and no other triggers or turbidity currents that occur without a clear trigger are considered. This excludes a bunch of new and often surprising insights from recent turbidity-current monitoring experiments that show that turbidity currents can be triggered even by dilute surface plumes and often occur without a clear trigger. I do agree that this particular system might be dominated by terrestrial debris flows but this does not allow to connect each non-Earthquake triggered turbidite to a single terrestrial debris flow within the past 4000 years. I explain my argument in detail citing the relevant literature below.

However, I still think that there is much value in quantifying the recurrence rate and thickness of such events in the record. Recent literature has shown that these can contain important information on sediment flux and sediment transport processes (citations in main comment below). I suggest to represent this lacustrine turbidite record as a proxy for sediment delivery by sudden events into the basin, rather as a 1:1 terrestrial debris flow record derived from lake turbidites. That includes rephrasing the title of the paper. This will prevent the overinterpretation of the lacustrine turbidite record to a single terrestrial sediment-transport process but still potentially demonstrate the interesting increase in turbidite frequencies and thicknesses in the 20th century.

I also have additional comments on

1. the robustness of the distinction of 'df turbidites' and 'eq turbidites' based on geochemical and grain-size profiles,
2. age & turbidite thickness & frequency uncertainties,
3. Sadler effect-related issues regarding the increase of turbidite frequency and thickness, and
4. the value of the sediment delivery ratio in this context,

which I also list below.

I recommend major revisions of the manuscript but, at the same time, want to stress the high quality of the data sets and the efforts made for analyses. Once the authors put potential causes of turbidite deposition into a more realistic perspective, this manuscript will make a relevant contribution to the field of geomorphology an sedimentology.

I hope the authors find these comments useful.

Best Wishes,
Anne Bernhardt

Main comment

The paper distinguishes between earthquake-induced and debris-flow related turbidites.

The evolvement of terrestrial and subaqueous debris flows into turbidity currents has been well established in experimental data and in field and subsurface studies (Talling, 2013). However, flooding events as triggers of turbidity currents are excluded as no permanent river flows into the lake. However, there are ephemeral streams discharging into the lake that could shed fluvial floods into the basin (e.g., Katz et al., 2015). I do not see a way how one would distinguish between these types of triggers based on the current data set.

The association of debris flows and turbidity currents has been shown in marine strata where marker beds and continuous outcrops have allowed bed-by-bed tracing (Amy and Talling, 2006). However, a clear association of debris flow deposits to equivalent turbidity currents has not been shown by the authors of this paper.

Moreover, turbidity currents do not necessarily need a specific trigger, piling up of loose sediment on the lake-margin slope and excess pore pressure will eventually result in failure and a turbidity current can evolve. Frequent turbidite activity with no major triggers has been shown by direct monitoring in the recent years (e.g., Clare et al., 2016; Paull et al., 2018). Furthermore, Hizzett et al. (2017) have shown that turbidity currents are most often triggered by settling plumes, rather than landslides or flooding events. I urge the authors to familiarize themselves with these recent research on triggering of turbidity currents, which has led to very surprising results and shifting paradigms over the recent years. I do agree that these experiments were carried out in different settings that the Plansee area, but they do show the complexity of turbidity current triggering.

It seems like the authors are 'automatically' categorizing turbidity currents as debris-flow induced when they were not classified as earthquake-triggered in a previous publication by Oswald et al. (2021). In line 447 and onwards, they state: "The potential of misinterpreting df turbidites as river flood-induced turbidites, which could have similar characteristics (Gilli et al., 2013; Wilhelm et al., 2013), is very low at this subbasin of Plansee. This is because possible hyperpycnal flows related to the main inflowing rivers are trapped either in Heiterwangersee or in the easternmost subbasin in Plansee and do not reach the studied main basin". I do not think that this holds true, turbidity currents in the marine realm can cross obstacles of several 100s of meters height and have run out distances of > 1000 km on very low slopes (Talling et al., 2007; Völker et al., 2008). They can sure pass from the easternmost subbasin into the main basin, a distance of only < 4km (but not from the Heiterwangersee to the Plansee basin, I agree). Surely, lacustrine turbidity currents are probably smaller than big marine events, but there is no basis to exclude that these run across the lake floor. Plus, turbidity currents – again - can occur without specific triggers.

I do agree that based on the abundance of debris flow lobes on the modern subaqueous fan delta, as nicely illustrated in the first part of the manuscript, debris flows are a very important sediment transport process in this area. However, given the complexity of subaqueous sediment failure in natural systems, one cannot assume in turn that every single turbidite (not assigned to an earthquake) has been triggered by a debris flow for the past 4000 years. As mentioned before, data from recent experiments that monitored turbidity currents directly also on deltas – do not show much evidence for this at all.

In order to go forward with this interesting record, I would distinguish between earthquake-induced and other turbidites. There is still lots of value in quantifying turbidite frequency and thickness as a general proxy for sediment input related to episodic sediment density flows (e.g., Covault et al., 2010; Bernhardt et al., 2017). Supposedly, many of these turbidites are related to debris flow events, but by no means can this record be used as a direct record for (onshore) debris flows only.

The title would need to be adapted.

The discussion section 5.4 should be reframed (and shortened) to 'Driving forces/ controls on turbidity-currents activity' and carefully avoiding overinterpretation of the record.

The robustness of the distinction of 'df turbidites' and 'eq turbidites' based on geochemical and grain-size profiles

Line 715 of the Conclusion states:

"Df turbidites show a graded grainsize trend, have a less steep D50/D90 ratio and contain more TOC compared to the homogeneous eq turbidites. „

In my mind this statement is not fully supported by the data shown in Figure 6.

The two distinctly looking grain size profiles of Fig 6a obviously do not prove that there is a statistical difference between the two types (if there are only 2 types of tubiidites, see my doubts above…) in terms of the grain-size grading of the deposit. The authors need to show that there is a robust difference in the grading of the two deposit-groups based on the analyses of many deposits.

In my view Fig. 6b shows no significant difference between the different turbidites types in TOC-d13C space. If the authors want to state a difference here, they need to show by e.g., a cluster analysis, that these clusters are actually statistically distinct and the two groups cannot be drawn from a single distribution. How does the sample size (many df , fewer eq and background data points) affect the analyses? The same applies to the data shown in figure 6c: Is the difference in D50/D90 space significant? How would acquiring more eq data points affect the result?

Age & turbidite thickness & frequency uncertainties

Unclear how the ^{137}Cs & ^{210}Pb data informed the BACON model in Fig S2. Did it?

The authors address the uncertainties in the age models by showing estimates for every single BACON simulation, a valid approach in my mind. However, this approach is only shown for the 150 yr bandwith, not for the 21 yrs. Hence, the uncertainties on this model cannot be assessed. Please add.

Can you comment on the uncertainties on (often very thin) turbidite thickness measurements due to the often diffuse upper boundary to the background sedimentation?

Moreover Table 1 shows a bunch of parameters for each 'phase' with no uncertainty estimates. These need to be added to make this comparison between 'phases' meaningful.

Difference in Phase 3 and 4 Fig 8a: Is the increase in cumulative thickness between phase 3 and 4 actually significant when the uncertainties are accounted for? Cannot tell from this plot.
Line 695 and ongoing:

„we infer a mean frequency of phase 4.1 and 4.2 to be a best estimate, showing a ~7-fold increase in phase 4.2 compared to phase 3 (~1520 to ~1920 CE) coincident with the instrumentally documented increase in rainstorm activity"

Interesting, but I cannot follow this argument as the rainfall data are not shown in your figures as only the precipitation record of the past 118 years is shown in figure 8b. This is an important observation, to convince your reader you need to show the data & the robustness of the correlation.

Sadler effect-related issues regarding the increase of turbidite frequency and thickness

Turbidite thickness and deposition rate is generally increasing towards younger deposits. Can these changes be simply attributed to the statistical bias describes as the Sadler effect as older strata is more prone to hiatuses and sediment removal (Sadler, 1981; Sadler and Jerolmack, 2014).

The value of the sediment delivery ratio in this context

Line 188, Terrestrial LIDAR data Equation 1

I do not understand how you determine the numerator: sediment flux into the lake, by 3 LiDAR surveys which are 3 month apart. What about the sediment flux from farther up the catchment that bypassed the survey area and ended up in the lake? Please clarify.

Line 395 states: During the 3 months, the sediment only accumulated on the terrestrial fan delta.

How do you know? Differential bathymetry on the lake bottom? Sediment traps employed? How do you now no sediment bypassed into the lake?
I guess your argument is the sediment delivery ratio. But (if I understand this correctly) that ratio is only based on data of the fan itself. However, material mobilized further up in the catchment could have simply bypassed the fan (eroded some, deposited some on the alluvial fan) and deposited in the lake. I do not think this ratio is useful to determine how much sediment made it into the lake during the 3-month period, if not the entire catchment draining into the lake is imaged & considered.

And one more:
Line 577 I am not sure you can use the sediment delivery ratio – the way you calculated this – as a measure for system connectivity, as there might be much more sediment involved from further up in the catchment making it into the that is not accounted for by your difference maps of the fan. I don't think this does much to your story. Would just leave this out and shorten the manuscript somewhat.

Landscape recovery from earthquakes

This is a very interesting aspect that may deserve more attention in some other manuscript. However , if I read figure 7a correctly – events during the "P-period' can only be recognized in the proximal cores Pan 19-03 and Plan19-04 because the two distal cores (Plan18-10 and Plan 19-02) do not penetrate deep enough to even capture these events. So how do you know these events are not recorded in these locations but were simply note penetrated by the two distal cores?

Minor comments:

It sure is a challenge to derive useful information of datasets covering completely different time scales. The 'amphibious' approach is much praised in the intro, however, what are exactly the new insights that we gain form this approach? This falls somewhat short.
In my view you can show that the currently active parts of the alluvial fan-delta system, that active delta front is dominated by debris flow lobes and erosion and deposition happens on the alluvial fan.

In line 98, the authors state that there is no permanent river inflow to the main basin of the Plansee. However, Line 112 mentions two permanent inflows /draining catchments) into the lake. Please clarify the discrepancy. By main basin you mean your specific study area? And these basins are well separated by a high morphologic barrier in between that turbidity currents cannot cross? Does not look like it in your figure 1…

Fig. 3 and line 590
What do you mean by overspilling debris flows. Spilling over what? Beyond the break of slope onto the lake floor?

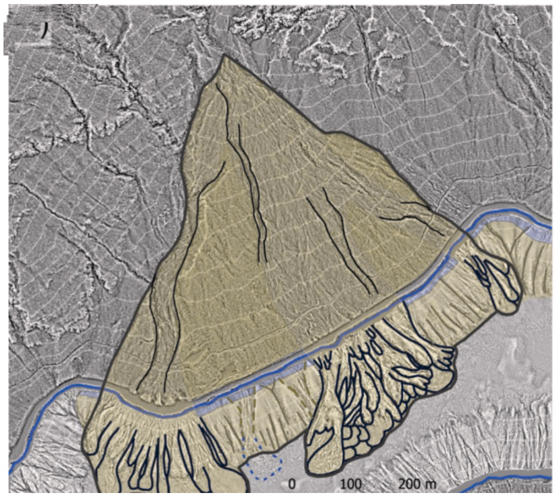
Line 119 states that
„While the human interference may influence the continuous background sedimentation in the lake quite a bit, the episodic debris flows eroding materials from 10,000 m2 large catchments way above the lake will not be relevantly influenced." How are you so sure? Is there no human activity at all in the catchments above the lake? Deforestation during historic times/ reforestation after etc?

Line 300 and ongoing.
It seems like the authors assume that debris flows are the only sedimentary process happening on an alluvial fan. What's your evidence for that? Are there no fluvial or sheet flow processes happening? 100% debris flow? Are these deeply enough incised (4000 yrs of stratigraphy) and these outcrops show only debris flow deposits and no fluvial processes?

Line 410
The Lightness L* factor of the sediment is first mentioned here but should be explained in the Methods section already

Avoid unnecessary repetition of words throughout the manuscript, e.g.,
Line 42 two times "worldwide"
Line 16 and 18: "amphibious" (is this a good word for what you actually mean – combined terrestrial and lacustrine data acquisition)

Figure 2d and others.
To ensure inclusiveness, make sure your figures are legible to people with certain color-blindness (8% of all men and 0.5% of all women are colorblind). I am not colorblind (I think ;) but the differences in color are hard for me to see. I used this website to check your Fig 2d
https://www.color-blindness.com/coblis-color-blindness-simulator/
and apparently the figure looks like this for people with Red-Blind/Protanopia & your differently mapped areas are undisguisable.
Enhance the contrast between the colors and this will work better.



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