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Interactive comment on "Subglacial carbonate deposits as a potential proxy for glacier's existence" *by* Matej Lipar et al.

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GENERAL COMMENTS Dear Editor, I've read the manuscript "Subglacial carbonate deposits as a potential proxy for glacier's existence" by Matej Liapr et al., submitted to the Journal The Cryosphere. This is a very important topic and I congratulate the authors for having had the idea of doing this research. The vision of a little ice age glacier-extension in the Alps as a short-time (few centuries) events, persisted for decades, but now researchers and climatologists, thanks to a continuously increasing number of datasets and proxies, are approaching a different perspective of alpine glaciation size during the Holocene. This is certainly a topic which will grow attention in the next years. I'm excited about future results which are really important especially for the understanding of the magnitude of present unprecedented abrupt glacier-decay

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observed in the Alps due to Anthropogenic global warming. Nevertheless, If I'm very happy with the topic, on the other hand, I guess this manuscript deserves more attention to how things are presented. The lack of discussion is evident at times, as well as a deeper analysis of the paleoclimate significance of the results with the huge amount of existing literature about LGM and Holocene glaciation. The English is good, although sometimes it needs a few adjustments

SPECIFIC COMMENTS

L26-27 When referring to the definition of glacieret a good reference is also the Unesco glossary of glacier mass balance and related terms by Cogley et al. available at this link https://unesdoc.unesco.org/ark:/48223/pf0000192525 Nevertheless, Serrano et al. 2011 gave a very interesting view of such minor ice bodies discussing their evolution from a disintegrating glacier or in areas where nival processes are dominant. To me, it would be important to add also this view in the introduction.

L 29-30 Pay attention, reference Bahr and Radić, 2012 should be highlighted after the sentence "occupy a significant volume fraction at regional scales" and not after "and are thus an important target for palaeoclimate studies"... they never stated this. Anyway, the sentence is overall questionable because the maximum size of the Triglav glacier during the Holocene was much larger than the size of a glacieret. I suggest rewriting the sentence in order to clarify this important aspect

Line 39-43 Subglacial carbonate crusts are also reported in the European Julian Alps by Colucci, 2016 (ESPL page 1232, Geomorphic influence on small glacier response to post-Little Ice Age climate warming: Julian Alps, Europe)

Line 51 You should be consistent with the given definition of glacieret, representing the actual state of this ice body. Honestly, as mentioned above, I would prefer the definition given by Serrano et al., 2011 and classify this ice body as a "glacial ice patch", meaning that it is actually an ice patch (no more than 2-3 m thick), residual ice body of a recently flowing glacier.

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Lines 84 and 94 Add space after “(sub)”

Line 127-133 I would suggest replacing here the term “glacieret” with “glacier” especially because when referring to the further chapter at line 135-138 is correctly stated that carbonate deposition resemble flow direction of the glacier and precipitation was strongly influenced by the mechanical force of the ice movement. Please, give clues about the location and number/name of such younger dated samples and of all the cited samples. It is important for the reader to understand where each dated sample is located in the surrounding s of the present ice patch which is a non-moving ice/firn mass. Nevertheless, after reading all the manuscript, I think this sub-chapter is rather unuseful and might be deleted because they are better presented and discussed in chapter 4

Line 174-175 In that paper Resfinder et al. stressed the fact that carbonate crusts were preserved by very cold and dry Arctic climate, which is really not the case of the Triglav cirque in the last century or during any possible period of absence or almost-complete-absence of an ice body. This is particularly true when looking at Mean Annual Precipitation (MAP) of 2600 mm w.e. recorded at Kredarica.

Line 182 I’m wondering if this could be entirely correct. At the LGM peak, Kuhleman et al2008 suggested a lowering of summer temperature of about 9-11 °C in the southeastern Alps. This roughly means that at Kredarica (2514 m), where the present summer temperature (1981âĂŤ2010) is around +6.0 °C (http://meteo.arso.gov.si/uploads/probase/www/climate/table/sl/by_location/kredarica/climate-normals_81-10_Kredarica.pdf) and considering the recent warming in the area calculated in the southeastern Alps in +1.7°C since the end of the Little Ice Age by Colucci & Guglielmin 2015, should have been roughly between -4.7 and -6.7. These characteristics lead to the existence at that location of a cold base glacier, instead of a temperate glacier. Nevertheless, given dates at 23.62 ka, 18.45 ka, and 12.72 ka suggest some interesting speculation. As stated by Monegato et al., 2017 in the paper “The Alpine LGM in the boreal ice-sheets game” published in Scientific

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Reports (https://www.nature.com/articles/s41598-017-02148-7), the LGM seems to be characterized by 2 main peaks with a withdrawal phase between 23.9 and 23.0 ka when tha Garda glacier retreated, which fit rather well with the 23-62 ka date given in this work. The final collapse of the Garda glacier occurred around 17.7-17.3 ka but soon before there was a progressive stacking of moraines related to the retreat and lowering of the ice surface which seems to fit well with the 18.45 ka dating. Both events could represent the occurrence of "some" subglacial water at the glacier-bed. Finally, I have no problem considering that during the Younger Dryas phase there was certainly a large amount of free water flowing at the glacier-bed. Small scale Detailed paleoclimate conditions in the Alps are still an open question, and uncertainties are evident especially in the eastern Alps when looking at bias between the modeled MIS2 stage ice extent (Seguinot et al., 2018) and geomorphological reconstruction (Ehlers et al., 2011), but the discussion could be expanded in such a way.

Lines 195-200 this is too speculative in my opinion. There is no evidence at present of cave ice older than roughly 10 ka at least in Europe, on my best knowledge. I would be more cautious in this manuscript deleting "perhaps even Last Glacial Maximum times"

Line 236 I would prefer "ablation" instead of "melting"

Line 241 I might agree with what it is here stated, but as a possible cause I would also cite the important work of Painter et al., 2013 (https://www.pnas.org/content/110/38/15216)

Line 242-248 This part is too hasty, although crucial in the discussion, and should be more deeply investigated and discussed. For instance, has been shown as the retreat of Triglav glacier since the LIA in the last century has been more evident than in other sectors of the Julian Alps where other glaciers existed. This is the case of Canin-Kanin or Montasio West glaciers which are lower in elevation than the Triglav glacier. The Montasio West is still classified as a moving glacier with dynamics due to internal deformation. The reason why Canin-Kanin and Zeleni Sneg (the largest LIA

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glaciers in the Julian Alps) had different fates in terms of shrinking velocity and a 200 m difference in the Equilibrium Line Altitude (ELA) has been justified by Colucci 2016 to a large difference in Mean Annual Precipitation (in the Triglav area precipitation are roughly 60% of that in Canin-Kanin) and potential annual solar radiation for the glaciers differed by about 7%. In this view Triglav glacier generally has higher sensitivity to summer temperature while Canin-Kanin lies in a more "maritime" environment and is more sensitive in changes of winter precipitation. I guess a discussion in terms of variability of these two parameters during the Holocene and/or in the Lateglacial period would improve this part of the manuscript. The literature is quite abundant on this topic.

FIGURES Figure 3 Is not adding anything crucial to the study area or the manuscript itself. It Maybe could be deleted and glacier outlines drawn in figure 2. Instead, Figure S2 could be added in the main article as Figure 3, maybe highlighting with arrows and numbers the location of the samples because in the main manuscript a picture of the study area with a view of the present state of the ice patch is missing

Suppl. Material Figure S3 . . . it would be useful to add a number of samples together with arrows Figure S5 . . . not clear which of the 3 caves is the Tiglavski Brezno Shaft. . . Fonts and size are not the best, please improve the size and the visibility of the text Figure S6 . . . I would change "melting season" with "ablation season". More, please give clues about the moving average used (how many years ?! It is centered ?) Figure S7 . . . Besides linear regression, I would also add a moving average which probably better highlights variability along about the last 170 years. These are indeed very interesting data, I'm asking my self if they are available in some repository to the scientific community.

Pointing out once more the importance of this in a way pioneering work, I think it deserves publication but after careful updatings and insights Dr. Renato R. Colucci, PhD

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