This work presents a nice summary of carbonate weathering and denudation using a compilation of 36Cl rates from around the world. Overall, the methods are sound and the results and discussion are interesting.

We thank the reviewer for this helpful feedback.

One particularly interesting result is that bedrock denudation and weathering rates inferred from rivers roughly agree, even though the timescale integrated by these measurements is quite different. The authors argue that this suggests that weathering dominates denudation of exposed bedrock (rightfully, I think). Does this also mean that weathering rates (at least on average) have remained relatively steady over the past several thousand years?

Yes, we assume that weathering rates have not changed significantly throughout the Holocene. This is highlighted by Figure 5, where we show the precipitation rates from three different paleo-climate models for the mid-Holocene and LGM. In general, the differences in precipitation throughout the integration time of cosmogenic nuclides do not vary substantially for most samples. Hence, we assume that weathering rates have remained relatively constant, at least throughout the Holocene.

Long-term denudation rates from catchments are faster, and the authors present a straightforward analysis that constrains physical erosion rates from these catchments. Weathering still makes up a substantial portion of the mass flux (much more than in silicate-dominated landscapes), and the authors conclude that carbonate landscapes should generally denude more evenly than silicic ones.

The weaknesses here lie mostly in some overlooked literature that should be included, and in some cases discussed. I'd also like to see a bit more information on the field sites, particularly the ones that make up the bulk of the analysis.

The literature that first partitioned denudation into weathering and erosion in silicic landscapes should be cited here. The approach presented here builds on that body of literature by applying these ideas to carbonates, but the approach itself isn't novel. This is also true of some of the weathering bias work (e.g. Riebe and Granger 2012).

We acknowledge that comparing CRN denudation rates to water chemistry measurements has been applied in silicate-rich locations. In the initial submission, we cited Larsen et al. (2014). In the revised version we added two references into the introduction (Blanckenburg et al., 2004; Dixon and Blanckenburg, 2012). We also added another sentence introducing literature and concepts applied to silicic landscapes. We cite from the revised introduction: "Studies applying a combination of CRN denudation rates and elemental analysis, e.g., measuring concentrations of immobile elements in the bedrock and regolith/saprolite have determined denudation portioning in silicic landscapes (Ferrier et al., 2012; Riebe et al., 2001b, 2003, 2004). However, these combined approaches have not been extended to carbonate landscapes, yet."

We are aware that some of the weathering bias approach used and cited in this study is similar to Riebe and Granger (2013). In the initial submission, we exclusively cited Ott et al. (2022) because the method of using stream water chemistry to correct denudation rates has only been published by Ott et al. 2022. Moreover, Riebe and Granger (2013) and earlier studies discuss the effects of quartz enrichment, and the compiled ^{36}Cl samples rather suffer from calcite depletion. However, we added a reference to Riebe et al. (2001a) to the introduction because this was the first study to correct the effect of quartz enrichment.

More information on field sites would be really useful, especially the Mediterranean ones that form the basis of much of the analysis. Is the bedrock ALL carbonate, or a mix of sedimentary lithologies? How big are the catchments?

This is a good point, and we have included this information at the beginning of the methods section. We cite from the newly added paragraph:

"Published bedrock denudation rates exist for Eastern Asia and the Mediterranean spanning a range of climate zones from the arid Negev desert in Israel to the humid mountains of Japan, with mean annual precipitation ranging between 190 and 2300 mm/a. Alluvial catchments span a narrower range of precipitation rates between 200 and 1100 mm/a and drainage areas ranging between a few km² in southern France to tens of km² in Israel and Crete, Greece (see Fig. S1 for catchment maps). Most of the samples in the compilation are from relatively pure, un- to moderately metamorphosed, massive, and bedded limestone bedrock (Avni et al., 2018; Godard et al., 2016; Ott et al., 2019b; Ryb et al., 2014a, 2014b; Thomas et al., 2017b, 2018; Xu et al., 2013). However, the bedrock samples also contain marbles (Matsushi et al., 2010), pure dolostones and chalk (Ben-Asher et al., 2021), and samples of unknown carbonate lithology (Yang et al., 2020)."

I find the argument that "most weathering happens near the surface" in a highly-reactive lithology, even in the presence of caves, to be unconvincing. Does the correction method of Ott et al 2022 include mass loss by weathering at depths > the attenuation lengthscale? Literature on the silicic rock community has identified deep weathering (which isn't "seen" by cosmogenic nuclides) as potential complication (e.g., Dixon et al., 2009; Campbell et al., 2022). If the correction accounts for deep weathering, it should at least be described in the supplemental material. I actually think it should be addressed briefly in the main text, but I understand that the authors may choose not to devote much space to it if it's covered in the previous publication. The summary in the supplemental material mentions changes in residence time due to differential mineral weathering (e.g. where quartz and carbonates are present together), but doesn't address weathering at depth.

Cosmogenic nuclides cannot account for mass loss below the attenuation length without adding elemental measurements from different depths. Therefore, our approach does not include deep weathering. In contrast to the silicic rock locations mentioned by the reviewer, the carbonate community assumes that due to the fast reaction kinetics, most of the carbonate dissolution occurs close to the Earth's surface. In the initial submission of this manuscript, we provided two references (Gunn, 1981; Worthington and Smart, 2004) with only minor context. To make a more convincing case and lay out the data that led to this conclusion, we have expanded this part of the method section. We cite from the revised manuscript:

"Erosion can be assumed to equal denudation minus weathering because, despite deep solution features such as caves, volumetrically, most carbonate dissolution occurs close to Earth's surface (Ford and Williams, 2010; Gunn, 2013). Field studies measuring water chemistry at different depths below the surface show that the most of weathering occurs within the first meters below the Earth's surface (Gunn, 1981; Williams and Dowling, 1979). Furthermore, studies quantifying the volumetric percentage of voids in carbonate bedrock found that only 0.003-0.5 % of the karst volume has been removed by deep dissolution (Worthington and Smart, 2004). Hence, we assume weathering rates primarily reflect near-surface mass removal."

Recent work from Erlanger et al (2021) should absolutely be cited here, and their findings should be considered in the discussion. They found a large fraction of dissolved load was actually re-precipitating as carbonate sand. If this were also happening in the catchments studied here, might the measured dissolved loads actually be a minimum estimate of weathering rates from rivers?

This is a good suggestion. The Mediterranean streams from which alluvial samples were taken are all ephemeral. In previous work by Ott et al. (2019), no secondary calcite coating was found in the alluvial samples. Due to similar ephemeral discharge at the sites in southern France and Israel, we assume that the precipitation of secondary calcite does not play a major role there either. However, it is an interesting point that the precipitation of secondary carbonates in wetter climates might effectively limit the amount of weathering export. We added to the discussion of the revised manuscript: "Additionally, Erlanger et al. (2021) showed that significant amounts of dissolved load could reprecipitate as secondary calcite in rivers flowing through carbonates, as soil pore water equilibrated to high CO_2 levels degasses upon entering streams. This effect has not been observed in the ephemeral catchments studied here but shows how in wetter regions erosion and weathering can be linked, and catchment and local scale weathering rates may differ."

It seems odd to add an example from outside the study (Ireland) in the final figure. I understand that the authors are trying to provide a low-relief end-member, and I recognize that there simply aren't that many places where 36Cl has been measured in catchments to compare to. Still, I suggest sticking to data reported/analyzed elsewhere in the paper to avoid confusion, rather than bringing in a new setting at the end.

We have removed the data point from Ireland from the revised manuscript.

Small tweaks for clarity:

Fig. 1: It looks like the sites from Ott et al 2019 are just catchments in the main figure, but like they're both catchment and bedrock in the inset.

We thank the reviewer for noticing this mistake and have fixed it in the revised figure.

I'd love to see a map with catchments in the supplemental – it's difficult to assess the size, gradient, topography, etc. when they're only reported as points at the sample location. At the very least, add catchment areas to the table.

We have added a map of the catchments for the three different regions in the supplement.

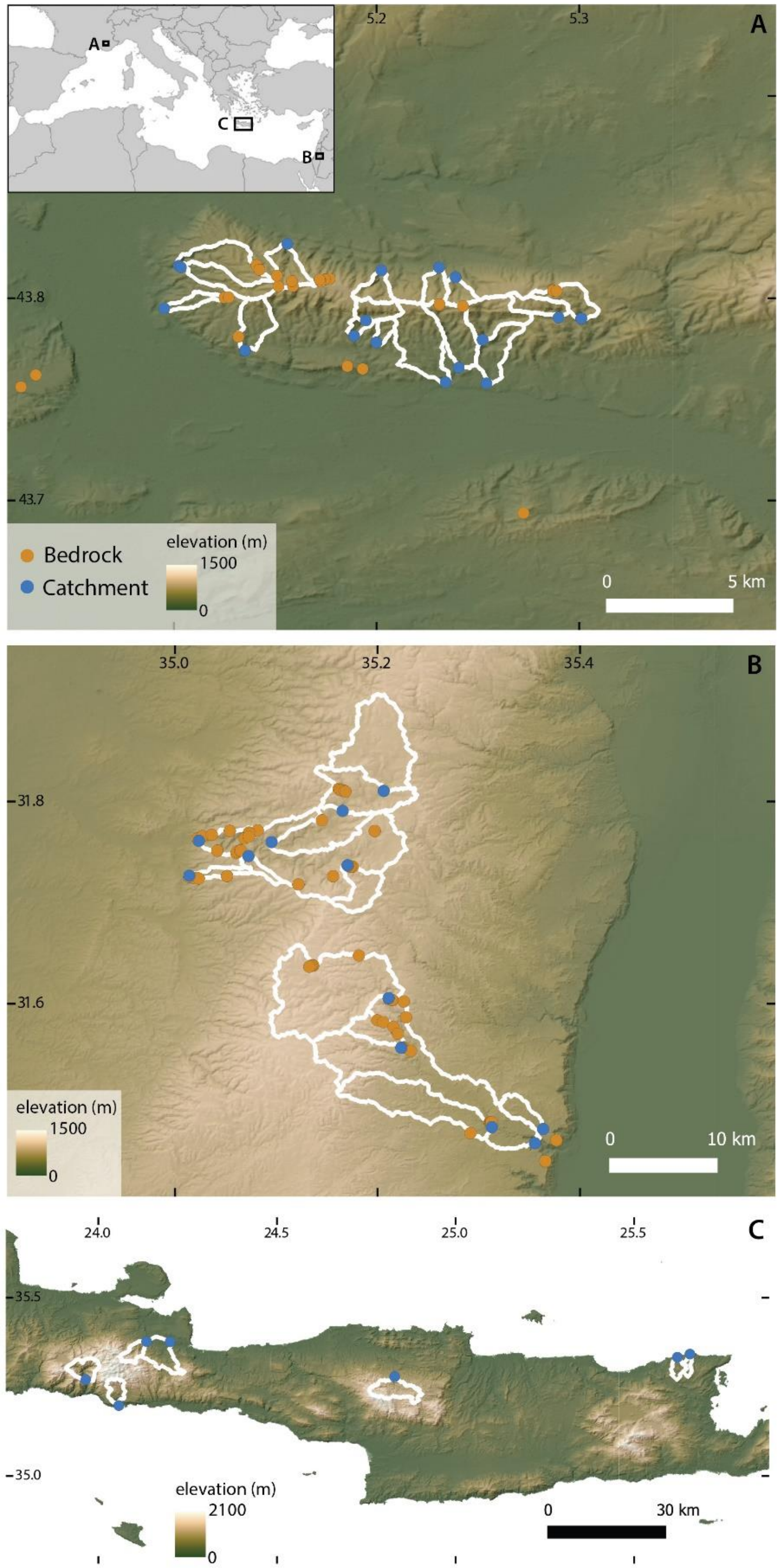


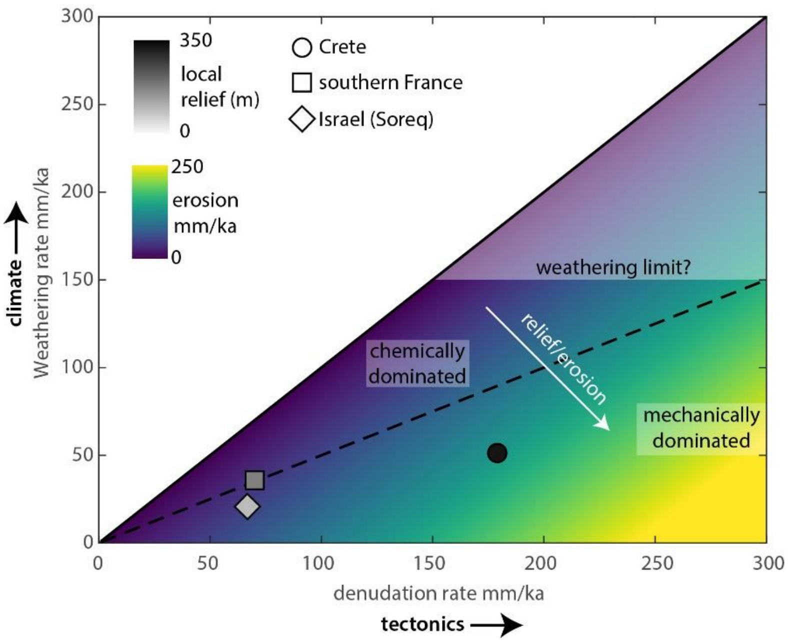
Fig. S1: Overview of catchments sampled for catchment-average denudation rates in southern France (A), Israel (B), and Crete (C), with nearby bedrock 36Cl sampling locations.

Section 4.4: It's hard to assess this info on these 3 sites without the context of relief or slope. It would be easy to add this info here, so readers don't have to go to the supplemental table to find it.

See reply to comment below.

Fig. 4: Color-code marker for sites (warm-to-cool?) in order of relief (or average slope). The red and orange dots are a bit close together in color tone, which makes them harder to distinguish on the figure. You might use a simpler color bar for the erosion rate gradient, perhaps? What's the dashed line?

The dashed line represents the 1:1 line of erosion and weathering. We have added an explanation to the figure caption. We have also included a coloring of data points by local relief.



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