Reply to the reviewers of **"Faunal remains from the Upper Paleolithic site of Nahal Rahaf 2 in the southern Judean Desert, Israel."**

We thank the recommender and the reviewers for their comments, which we accepted without exception.

Reply to Ana Belén Galán

1) I would just suggest rephrasing the title of the section "Discussion" as "Discussion and conclusion".

   Done.

Replies to *Joana Gabucio*

1. In the Materials and Methods section (p.5, line 160), the authors reported that all the faunal remains from the site were collected by dry sieving the sediments. This assumes that no Cartesian coordinates are available for the bones. It would be interesting to clarify if, as NR2 is a recently excavated site, the remains have some spatial reference (for example, a classification by m²) or not.

   Contextual control of recovery utilized a 1 sq m grid that was further subdivided into 0.5 sq m squares. This resolution has been maintained during dry sieving, and therefore we can trace each specimen to its position within the 0.5 sq m grid. We did not collect three-point coordinates for each bone specimen due to the density of finds. We added a clarification of this to the Methods section (line 161).

2. The authors evaluated bone preservation at the site by correlating bone mineral density for caribou provided by Lyman (1984, 1994) and the MAU values of the NR2 assemblage for each scan site. The technique used by Lyman to measure density values, Photon Densitometry, does not take into account neither the external morphology nor the internal cavities of the bones. Subsequent studies have worked to overcome these limitations, as well as to expand the list of scanned taxa (Kreutzer, 1992; Lyman et al., 1992; Elkin, 1995: Cruz & Elkin, 1995; Lam et al., 1998, 1999; Stahl, 1999; Pavao & Stahl, 1999; Dirrigl, 2002; Symmons, 2005). I would recommend the authors to use the density values calculated by Lam et al. (1999) for Rangifer tarandus using the Computed Tomography technique, which starting from similar scan sites exclude internal cavities of long bones in the calculation of density (Lam et al. 1998, 1999, 2003). From my point of view,

these density values are more suitable for their correlation with the skeletal representation of archaeological sites, especially in assemblages where diaphysis fragments of appendicular bones abound, as in the case of NR2.

We thank the reviewer for this comment. Lyman's photon densitometry values were replaced by the CT derived density values in Lam et al. 1999 (table 1, for *Rangifer tarandus*)(lines 201-204). Using these values, the correlation between density and MAU loses statistical significance and effect size (Spearman r = 0.21, p = 0.07)(lines 201-204; 336-342; reference added to the bibliography, lines (537-538).

3. I agree with the authors in the use of the nonparametric Spearman's rho test to correlate density measurements with the MAU. However, I do not think that a coefficient of 0.48 can be considered of moderate intensity (p. 6, line 204; p. 15, line 342). Taking into account that a coefficient of 0 would mean the absence of correlation and that the coefficients 1 (positive relationship) and -1 (negative relationship) would indicate a perfect correlation, the value of 0.48 rather reflects a weak intensity.

We are not sure that we agree about this. The guidelines offered by Cohen (1988: 79-81), considered authoritative (Hemphill 2003), would suggest a medium to large effect size for r = 0.48. The disagreement, however, is neutralized by Dr. Gabucio's preceding comment and the change of *r*, in our case, to 0.27 (a weak effect).

Hemphill, J. F. (2003). Interpreting the magnitudes of correlation coefficients. The American Psychologist, 58(1), 78–79.

Cohen, J. (1988). Statistical power analysis for the behavioral sciences (2nd ed.). Hillsdale, NJ: Erlbau.

4. Although the authors have previously used Spearman's rho test to correlate the MAU of the zooarchaeological assemblage with density measurements, when comparing fragmentation intensity with marrow and fat utility indices, they chose parametric statistical methods (p. 16, lines 372-375). It is true that some researchers use parametric methods to deal with utility indices (Binford, 1978; Metcalfe & Jones, 1988; Jones & Metcalfe, 1988). However, like many other authors (Lyman, 1985, 1994; Brink, 1997; Morin, 2007), I consider the use of non-parametric methods such as Spearman's rho and Kendall's tau to be more appropriate. For this reason, I suggest to the authors the use of non-parametric methods in this case as well.

We accept the comment. The OLS regression on marrow index has been replaced by Spearman's r (line 369, yielding Spearman's r = 0.29, p = 0.40, which is similarly weak and insignificant). The Pearson correlation coefficient has been likewise replaced by a Spearman's r (line 371), to identical results.

5. In figure 1, the stratigraphic section (B) is difficult to understand due to the small size of the image and the font. The figure would be greatly improved if this part could be enlarged a little.

The image has been enlarged.

6. In Table 1, it would be useful to add the percentages (%) of the different taphonomic modifications (although the numbers of altered remains are low), thus facilitating the comparison between levels.

Percentages were added.

7. I find the proposal to be very suggestive in that, through time, and as the environment slid into dry glacial conditions, hunting forays become more specialized (focused on prime adult caprines) and of shorter duration. While the proposal for an increase in specialization is well supported by data on the age at death and the evolution in the proportion of goats and gazelles, I think that the idea of more ephemeral hunts needs further discussion. The high frequency of weathered remains in the upper levels, in my opinion, is not a sufficient criterion, since it is more related to sedimentation rates (which affect the time that already deposited items are exposed on the surface, before being buried) that with the duration of the occupations themselves.

Our reasoning is that when humans occupy the rock shelter more intensively, they create deposits (garbage, ash, etc.) that result in quick burial of the remains and – hence – lower degree of weathering. While we think that this is a valid point for the discussion, we agree with Dr. Gabucio that the way we phrased the point (lines 431-432) has been somewhat misleading, and especially our use of the word "sedimentation". We tried to clarify this.

8. Finally, a review of the references has revealed the absence in the References section of some works cited in the manuscript:

- Alex et al., 2017

- Andri et al., 2021 (in the References section appears as Andri 2021)

- Bar-Yosef & Belfer-Cohen, 2010

- Belmaker & Bar-Yosef, 2011 (in the References section appears as Bar-Yosef & Belmaker 2011)

- Gilead, 1981

- Kadowaki et al. 2016

- Klein, 1995

- Marks, 1981

- Marder et al., 2020

- Orlando, 2019; Orlando et al., 2009

- Sarig et al., 2020 (is this Sadhir et al. 2020?)

- Steiner, 2005

- Stiner et al., 2005 (in the References section it appears Stiner 2005)

- Tejero et al., 2020

Corrected.