Reviewer #1

#RC1.1 Based on my personal reading, the presented analysis is of interest for hydrological modelling of ungauged catchments. I believe that the manuscript is well written and organized, and it deserves to be published in NHESS, after minor revisions listed below.

We thank the reviewer for this positive feedback and the different comments that will help to improve the manuscript.

#RC1.2 Line 56: "less than 200 km2" … you mention 300km2 at line 69

Thank you for this comment. We have replaced 200 by 300 at this line. The majority of the catchments have an area smaller than 200 km^2 but indeed a few of them are in the range 200-300 km^2.

#RC1.3 Line 66. I suggest to add (in this section 2.1 or at least in the supplemental) a table with the list of the catchments with their main features (numbering, identification name, area, elevation range or average value, …). If possible, consider to report the number of each catchment in the map in figure 1, and in the explanations of results and discussion together with the catchment name. This could help the reader in identify the catchments and their main features when reading the following results and discussion section. I was sometimes lost with the different names of the catchments.

Thank you for this suggestion that would help to follow the interpretations of the results. We have added numbers associated to each catchment in Figure 1, along with their names on the right panel. We have also added the Table 1 that provide some features of the catchments.

#RC1.4 Line 225. In all the equations in section 4.1, are you using absolute differences?

In all the equations in Section 4.1, we are using absolute differences as opposed to square differences in order to avoid inflated influences of the largest values. For some criteria (quantile relative error, volume error), these absolute differences are divided by the observed criteria and are in the end relative differences.

#RC1.5 Line 251-258. You define some errors (let's say E, where E = PFE, TPE, VE) using absolute differences, then you transform them as 1- E. I have to comments here: i) why not just considering E with sign, for having a measure of the direction of the error (under- or overestimation)?; ii) I found confusing the use of both E and 1-E in the explanations of the results (for example, lines 279-289). I suggest to use just one.

Thank you for this comment. Concerning the first comment i), the idea was to obtain criteria that are all positively oriented, as a mixture of positively and negatively oriented criteria can also be confusing. However, considering the second comment ii), we understand that the explanations are not clear with the interpretations of both raw and transformed criteria. We have kept only the raw criteria in the revised manuscript and we

now indicate clearly if they are positively or negatively oriented in Figure 3 (with red arrows).

#RC1.6 Line 270-275. i) For better compare the results, I suggest to use same y-axis limits for the plots referring to the same type of index. For example, for mNSE in panels a-d, for QRE in panels e-h, … ii) maybe a comment is needed about SMASH model in panel d), showing huge range of mNSE compared to MORDOR-SD.

We thank the reviewer for these constructive comments. Following comment i), we have modified Figure 3 to have the same y-axis limits for similar criteria. We agree with the reviewer that it will help the comparison of the performances for different hydrological signatures. Concerning comment ii), a small paragraph has been added: "*For these criteria, SMASH leads to large variations of performances across the catchments compared to MORDOR-SD. As discussed in Section 5.6 and shown in Figure S38 in the Supplement, interannual streamflows are under-estimated by SMASH in winter and spring for many catchments and overestimated in summer and autumn*".

#RC1.7 Line 439: You mention some problems in high-elevation catchments, and (line 468) that COMEPHORE underestimates precipitation in high elevation areas. Consider to add a plot of mNSE vs elevation, for example as panels in figure4, to synthesis the maps and made more evident (if there is) a relation of mNSE with elevation.

Thank you for this suggestion. Please note that even if COMEPHORE underestimates precipitation in high elevation areas, this does not translate into weaker mNSE values since these underestimations can be compensated by applying correction parameters, as explained at l. 472-474 of the manuscript. The figure R1 below shows the relationships between the different mNSE criteria and the elevation. There are no clear positive or negative relationships. In winter and spring, mNSE(Q) seems to exhibit a positive trend due to large variations of the criteria for low elevations. In summer and spring, the scatterplot seems to indicate a negative trend for elevation greater than 1000 m but this impression is mainly due to weak mNSE values obtained with SMASH. For the mNSE applied to the 10 largest floods (Fig. R1c), it is clearly highlighted that the floods are not well reproduced for some catchments located at high elevations, e.g. for the catchment L'Arve@Chamonix-Mont-Blanc. As these results do not seem to add more evidence of these relationships, they have not been included in the revised version.
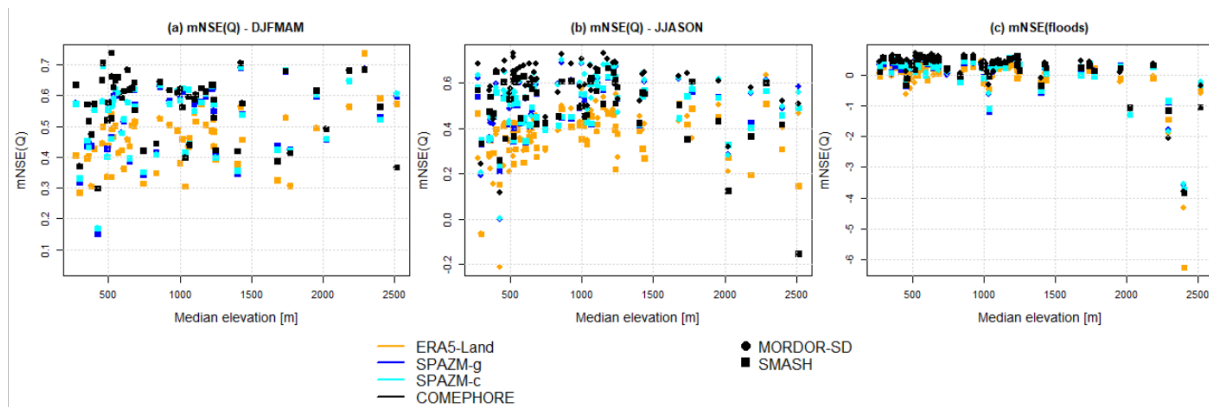

Figure R1. mNSE criteria as a function of the median elevation of the catchments, for the four different precipitation reanalysis and the two hydrological models. (a) *mNSE* of the

streamflows Q in winter and spring. (b) *mNSE* of the streamflows Q in summer and autumn. (c) mNSE for the 10 largest floods.

#RC1.8 Line 457: "small mountainous" maybe is "small catchments".

Thank you for noticing this missing word, this has been corrected.

#RC1.9 Supplemental, Figure S36-37. Same color scales in the panels could help in the comparisons.

Thank you for this suggestion, we agree that it will help the comparisons between these two figures and this has been modified.

#RC1.10 Supplemental, line 16. "than with COMEPHORE" is maybe "MORDOR-SD".

Thank you very much for your careful reading of the supplements and for noticing this mistake, this has been corrected.

Reviewer #2

We thank the reviewer for this overall positive opinion on this study.

It is true that many papers have investigated the impact of input resolution for hydrological modeling, usually based on dense gauge networks (Dong et al., 2005; Meselhe et al., 2009; Bárdossy and Das, 2018; Zeng et al., 2018). For example, Xu et al. (2013) show that the increase in gauge network density can improve the performance of the hydrological model. For a large basin in China of 94,660 km^2, the sensitivity of the performances to the gauge density reaches a threshold when the number of gauges is high (greater than 100 gauges for this catchment). Similarly, Emmanuel et al. (2017) show that higher spatial resolution of rainfall leads to better hydrological model performance for 25 catchments with areas ranging from 42 km² to 1,855 km². Huang et al. (2019) also find that a higher temporal resolution of rainfall improves the model performance if the station density is high and that an increase in spatial resolution does not improve significantly the performance of the hydrological model for four German catchments with areas ranging from 417 km² to 1300 km^2. Using synthetic rainfall fields, Zhu et al. (2018) show that the spatial resolution of precipitation has a larger impact than the temporal resolution for the simulation of floods but also depends strongly on initial soil conditions. Compared to the existing literature, a major difference in our study is that only small catchments (< 300 km^2) are selected and, in addition, which are located in mountainous areas where it is not known that precipitation measurements are more scarce and uncertain, particularly at high elevations. The interaction between input resolution and model performance is thus exacerbated in our study, compared to studies where catchments are often located in plains with larger areas (typically up to 1,000 km^2). A few papers studying the impact of rainfall resolution are focused on small urbanized catchments (Cristiano et al., 2016) or small lowland catchments (Terink et al., 2018; Hohmann et al., 2021). A small paragraph with some of these references has been added to the introduction at l.33-35.

Downscaling methods and conditional simulation can be applied to obtain meteorological forcings at the appropriate resolution for the hydrological model. These approaches can be applied to disaggregate precipitation data temporally (Parkes et al., 2012, Breinl and Di Baldassarre, 2019) and/or spatially (Bárdossy and Pegram, 2016) which is also a strategy applied in our study. Some references have been added to the manuscript in Section 2.2 (l.86-88). An important challenge for the disaggregation is the need of a

meteorological product at a fine resolution to establish the relationship between large-scale and small-scale meteorological statistics.

#RC2.4 It appears to me that an essential modelling choice is missing: how to combine the meteo product with the model? There is one option presented per hydro model but we do not know if this is a heuristic choice or the best option or what the literature says about this. In general, I do e.g. not think that retaining simply the meteo pixels within a catchment is the best option (but perhaps it is for rainfall?).

In our knowledge, taking the average of the meteorological inputs over the pixels covering the catchment in order to estimate areal meteorological forcings is the standard approach when lumped hydrological models are applied. There exist alternative approaches when meteorological data has a coarse resolution but there are not relevant here since the reanalysis considered in this study have a fine resolution.

#RC2.5 There is a short discussion on the absence of a precip gradient for one product but perhaps it would be good to have a more systematic discussion of how to create the hydro model meteo based on the input meteo.

Thank you for this comment. In this study, it is true that we mainly discuss the relevance of the meteorological products for hydrological modelling. How the different hydrological models process the meteorological products is a separate issue. In our case, MORDOR-SD assumes that precipitation and temperature have a linear relationship with the elevation, using orographic gradients (parameters `gpz` and `gtz`, respectively). There are used to provide precipitation and temperature data for different elevation bands. SMASH is a distributed model and directly takes as inputs what is provided by the meteorological data. These aspects are model specific, along with the choice of the parameters (fixed/inferred), the processes which are represented, the data used to estimate the free parameters, etc. In this context, it seems difficult to provide a systematic discussion on these aspects.

#RC2.6 Also, I strongly recommend to make much clearer what take-aways are relevant beyond the studied catchments and how new they are (at the moment, there are two take-aways which do not reveal completely new insights).

It is true that some important and general messages could be provided in the conclusions. A first important message is that ERA5-Land or satellite-driven reanalysis are not likely to provide fine-scale precipitation dynamics which are necessary for the hydrological modelling of this type of small mountainous catchments. While the limitations of ERA-Land have been discussed by a few papers (l. 209-211 of the manuscript), the implications of these limitations for hydrological applications are absent from the literature in our knowledge.

The other key message concerns the added-value of radar measurements for the hydrological modelling of these small mountainous catchments. In our case, the underestimation of annual precipitation amounts at high elevations was a known deficiency of the COMEPHORE reanalysis. Our study reveals that despite this limitation,

COMEPHORE provides better performances for some events because it provides additional information concerning the spatial structure of the precipitation fields. However, this key message is really specific to this reanalysis and for the region of the study, since COMEPHORE strongly depends on the availability of radar measurements (see Section 5.4). Beyond the studied catchments, a general message is that radar measurements are interesting to incorporate for the hydrological modelling of small mountainous catchments despite their limitations (radar signal attenuation by precipitation or beam blockage). While it has been shown for some flood events (Delrieu et al., 2005; Borga et al., 2007), our study provides a comprehensive evaluation of the added-value of radar data in this context, for many catchments and with two different hydrological models. The main conclusions have been rephrased in the revised version of the manuscript (Section 6).

#RC2.7 What does all this for the modelling of ungauged catchments, for which no correction parameters can be calibrated? Would be cool to have some input on this question.

Thank you for this comment. This is a very important and difficult question. In this study, correction parameters are applied mainly to balance amounts of water at aggregated scales. Therefore, it can correct different aspects that affect the water balance: lack of precipitation or precipitation excess, groundwater exchanges, impact of the karst. As such, these parameters are very difficult to regionalize since they are related to different characteristics specific to each catchment and/or to the meteorological forcings for which we have incomplete and uncertain information. This issue remains the main unresolved challenge for the application of hydrological models (conceptual models or other types) to ungauged catchments, before the representation of the hydrological system.

#RC2.8 From the abstract, the actual innovation is not clear, it seems like another paper on a previously often studied topic; what is small?

We now precise that "small" refers to catchments with an area smaller than 300 km^2 in the abstract. We are not aware of similar studies comparing very different precipitation reanalysis for the hydrological modelling of many small mountainous catchments. Studies which apply hydrological models to small catchments are usually restricted to a few lowland or urban catchments, and usually consider meteorological forcings based on gauged data only (see response to comment #RC2.2).

#RC2.9 Very few references at the start of the intro, little reference to extensive literature on the role of spatial resolution of input (rainfall) forcing on the quality of hydrological simulations.

See our response to the comment #RC2.2.

#RC2.10 From the methods, I understand that the spatial input product is aggregated to the hydro model by taking the meteo values per pixel: is this a good strategy given that the meteo product does not represent the true topography of the catchment ? In particular for the model that uses elevation bands? How are the pixel of the meteo product matched with the pixels of the hydro model for the other model?

See our response to the comment #RC2.5.

#RC2.11 From the methods it seems like the two models are not calibrated with the same criterion, why? Is this a good idea? does this impact the analysis beyond what is mentioned in the paper?

We agree that the choice of different criteria for the optimization of the two hydrological models impacts potentially the modelled streamflows and the corresponding results. In this study, it was decided to have two different hydrological models applied with the choices made by the developers of the two models (the choice of the parameters (fixed/inferred), the processes which are represented, the data used to estimate the free parameters, the input processing, etc.

#RC2.12 The paper would benefit from a concise summary of how the two models differ in terms of process representation? How do they estimate evaporation? Evaporation is almost not mentioned in the entire paper? But it should have a key influence on the representation of the water balance?

We agree that the description of how each model estimates the evapotranspiration is missing form the current manuscript and it has been be included. For MORDOR-SD, potential evapotranspiration (PET) is estimated using the formula provided by Oudin et al. (2005). The actual evapotranspiration is estimated according to the PET and different model states. SMASH has a similar formulation which is now precised is the corresponding appendix B.

#RC2.13 Section 3: how do the model topographies of the meteo products differ?

See our response to the comment #RC2.5.

#RC2.14 Section 4: do I understand correctly that the models are calibrated with criteria that are discussed in the sections dedicated to the models and that other criteria are used to evaluate the performance? Or are the criteria in the model section not relevant?

ifferent criteria are used to calibrate the hydrological models and to evaluate their performance. The set of criteria used to calibrate the models is limited and has a primary objective to provide constrained estimates of the parameters and avoid parameter equifinality. The set of criteria used to evaluate their performances is a lot richer in order to obtain a comprehensive assessment of the performances.

#RC2.15 Are the models calibrated with each meteo product?

Yes, this is correct. This has been clarified in Section 4.2.

#RC2.16 And why is the error on the floods not simply computed as a square-error, is NSE appropriate for this kind of signal? Are the values comparable to those of an entire year?

Mean square errors depend on the magnitude of the streamflows, which can be very different from one catchment to another. The NSE is comparable to other criteria (R square, mean relative errors) and the aim here is to intercompare their values considering different meteorological forcings or hydrological models.

We agree with the reviewer that the mNSE values are not comparable for the different catchments. Here, we want to intercompare the mNSE values with different meteorological forcings or hydrological models and see if systematic differences can be observed.

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Reviewer #3

We thank the reviewer for this positive feedback.

Thank you very much for this constructive comment. We agree with the reviewer that the outcomes of this study are not limited to the choice of the best precipitation reanalysis since three of them are only available in France. The primary objectives have been rephrased in the revised version of the manuscript in order to match more closely one of the key messages: the added-value of radar measurements for the hydrological modelling of small mountainous catchments (see l.62-66 of the revised version).

We agree than l.18-19 of the manuscript could be detailed in order to discuss the quality of radar measurements for precipitation in mountain regions. This is discussed in details by Germann et al. (2006) and by Villarini and Krajewski (2010), who indicate that the most important limitation of radar measurements in mountain regions is due to beam blockage (see the section 2.4 in Villarini and Krajewski, 2010). Another specific challenge is related to the phase change of precipitation that often occur between detection and arrival at ground level, potentially within/below radar elevation, which attenuates the signal within the melting layer, as shown by Khanal et al. (2019) in the northern French Alps (see l.19-21 of the revised version).

A part of the literature dedicated to quantitative precipitation with radar remote sensing in complex terrain focus more specifically to the estimation of snowfall using ground measurements of snow accumulations (Rasmussen et al., 2003; Von Lerber et al., 2018). While unique reflectivity/rainfall Z/R relationships can sometimes be applied to obtain rainfall estimates, snow estimates using radar data require different reflectivity/snowfall Z-S relationships for the different types of snow (dry/wet snow) and other factors (crystal type, degree of riming and aggregation, density, and terminal velocity, see Rasmussen et al., 2003; Khanal et al., 2019). As indicated by Von Lerber et

al. (2018), an additional limitation of snowfall estimates using radar data is related to the fact that Z/S relationships strongly rely on ground measurements of solid precipitation. However, these ground measurements are uncertain and are known to suffer from marked undercatchment. We briefly mention these aspects in the revised version. Please note that in our study, COMEPHORE does not have dedicated estimates of snowfall. Therefore, a more detailed discussion of these technical aspects seem out of the scope of the paper.

#RC3.4 I found the most interesting part of the paper to be the discussion. Specifically, the analysis of model performance with different products is linked to process representation in the model (groundwater loss) and precipitation and elevation representation. I would like to see some more information on radar performance for snow vs. rain at higher elevations, and if that could cause some of these issues.

Figure 8 presents the relationships between the correction parameter of the model MORDOR-SD and the elevation. As was indicated in the paper at l. 353-355, COMEPHORE seems to underestimate precipitation amounts in high-elevation catchments (cp > 1), since a correction parameter greater than one is likely to indicate an underestimation of the total precipitation. This underestimation was explained in section 3 at l. 216-218: "A*s COMEPHORE does not integrate any additional constraint about the effect of the relief, the vertical profiles of annual precipitation amounts are almost flat*". The underestimation is not well understood but is probably related to the effect of beam blockage and to the different postprocessing steps applied to obtain the final precipitation estimates using the raingauge network. In the French Alps, Faure et al. (2019) provide some explanations for a similar radar/raingauge product provided by Météo-France (PANTHERE). They show that overestimations at low elevations and increasing underestimation at high elevations can be related to the altitudinal gradients of precipitation observed at ground level. We do not have specific results about the evaluation of COMEPHORE according to the precipitation phase but Faure et al. (2019) provide some evidence that the general underestimation of precipitation from COMEPHORE at high elevations has probably little to do with the type of meteors.

#RC3.5 Fig 8b also suggests that ERA is actually quite good at capturing high-elevation precipitation, which is a strength that could be mentioned in the conclusion.

It is true that in this study, high-elevation total precipitation at an annual scale is actually well represented by ERA5-Land as indicated in Fig. 8b and it has been added in Section 6 "Conclusion".

#RC3.6 The groundwater section could also be clearer: Do you mean water is exiting the basin as groundwater, so you have to reduce precipitation? It would be interesting to have more information on how this lack of process representation could be fixed, and what would be advantages of using a model with groundwater processes included in the study.

Yes, we mean that it can be suspected that water is exiting the basin as groundwater so that the total precipitation amounts must be reduced to improve the water balance. It is

now clearly indicated as a likely explanation for some catchments of the study areas located in the plains around Valence and the northwest of Grenoble. However, this is a speculative interpretation, as we do not have much information about the groundwater processes of these catchments. A line has been added in Section 5.1: "A possible explanation is that water is exiting the basin as groundwater.".

MORDOR-SD can apply specific parameterizations in order to include a conceptual representation of these groundwater processes. However, after preliminary tests, we opted for a simpler application of MORDOR-SD which avoids catchment-specific parameterizations. This was done to simplify the main messages, and for the sake of comparison between the two hydrological models.

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