Editor and Reviewers comments:

Our responses to the comments provided by the editor and both reviewers are shown in blue.

Comments to the Author:  
  
Dear Authors,

Two leading experts have reviewed your manuscript, and they found it a beneficial contribution to understanding better how base water potential for seed germination can be utilised as potential traits for understanding the seed functional ecology of a species. They found novelty in your approach to studying this unexplored aspect of plant regeneration ecology and appreciated your efforts in conducting detailed experimental studies. I agree with the reviewers that your study is novel and will become a reference for future research. However, some sections, especially the methodological and result sections at several places, are hard to understand quickly, and reviewers need to dig deeper to get a better understanding, thus needing revision (see detailed comments of reviewers). It is, therefore, suggested that a significant revision be made and the critical comments of both reviewers addressed.

Dear Editor,

Thank you for considering our manuscript. Please see below our responses to the comments made by the reviewers. We considered all specific comments and suggestions and we have incorporated them into the revised version.

We hope that you find this version acceptable for publication in Functional Ecology. All authors contributed to and approved this revised submission.

Yours sincerely,

Clara Espinosa del Alba (Corresponding Author) and collaborators.

REVIEWERS' COMMENTS TO THE AUTHORS  
Reviewer: 1  
  
Comments to the Corresponding author

This is a very interesting study that brings new attention to an overlooked functional trait (base water potential for germination) that could have consequences for how species respond to a changing climate. Base water potential for germination is a challenging trait to estimate, both from an experimental and a modeling perspective, and I appreciate that this study is accessible – the writing is generally very clear, and they take advantage of an R package that simplifies the estimation process for base water potential. Moreover, they test a question that I’ve never really seen evaluated before by asking whether seed germination response traits can vary over small spatial scales (10m) in correspondence to microsite differences in a heterogeneous environment. For a seed, 10m can be a very large difference in terms of the experienced microenvironment, yet we have very little understanding in the field of ecology of whether and how microsite variation leads to different eco- and evolutionary processes during regeneration, or scales up to influence community structure and response to larger-scale climatic changes.  
  
I would like to see this paper be published and am hopeful that it will generate increasing interest in the functional significance of germination responses. However, I have a few questions about the analysis and think that the manuscript would benefit from a bit more detail about the hydrotime metrics and more nuanced, comprehensive interpretation of what the observed patterns might mean, ecologically. I note these questions and suggestions below and hope they will be helpful in strengthening an important manuscript.

We were very pleased to see that Reviewer 1 had a positive view of our research and valued the novelty of the question and the approach we used. We deeply appreciate the additional time dedicated to reviewing this manuscript.

Comments in the PDF:

L21-22 “These results support that intraspecific variation in the ψb for germination has functional significance” --  I think this wording is a little strong. Demonstrating that trait variation exists, and is observationally tied to the environment, is important but still not a direct test of functional significance, in my mind. To demonstrate that, I think we would need a subsequent empirical tests of germination and recruitment outcomes in the field to demonstrate that differences in base water potential lead to differences in performance or fitness in the environment. Studies from other semi-arid systems have suggested that variation in base water potential may exist, but is not necessarily a strong driver of differences in germination (relative to temperature) – and that predictive models assuming a common base water potential for all species (i.e. a ‘wet-thermal model’, assuming no variation in base WP) capture most of the expected variation in germination generated by fully hydrothermal models that take variable base water potential into account (e.g., introduction of Hardegree it al. 2018: <https://doi.org/10.2135/cropsci2017.11.0666)>

We read the paper suggested by the reviewer and include it as another discussion point in L 477-482.

This is not to argue that variation in base water potential doesn’t matter – on the contrary, I think this paper is very timely because we have not fully examined this question and utilized field experiments to understand how it does matter. Rather, I think the results presented here are a compelling first step and suggest that this trait could be functionally significant in this system, but that additional work is still needed to demonstrate that and understand how (And this future work would be very valuable to the field!).

We agree with the comment made by the reviewer. We have rephrased the specific sentence L 22 that Reviewer 1 highlighted but also made small changes along the manuscript. See changes in L 381, 409-410, 439, 453, 502, 504.  
  
L24 Again, I think these patterns suggest that this trait may have consequences for fitness, but the results don’t demonstrate that outright... especially when we consider that we also do not know if this variation is heritable and adaptive from the current study.  These data only demonstrate an observational correlation between trait and microclimate, which is a very interesting place to start. So I suggest being more careful about language and interpretation throughout the manuscript.

We agree, changes made across the manuscript see specific lines above.  
  
L79 I might replace ‘largely’ with ‘likely’, as regenerative responses may depend on many different processes, not just germination response

Done.  
  
L214 Replace ‘has’ with ‘have’

Done.  
  
L212-220 I’m a little confused about the soil moisture data. In the methods above, it sounded like you had two gypsum sensors per each of four sites (8 total), but one of them was damaged (I imagine this is why there are only 7 datapoints). But in Figure 3B there are no data from Cañada but four datapoints from Penouta. Is this just an error in the coloring of the figure?

Thank you for the comment we provide more clarification below, and in text, and fixed the wrong color palette. Each Microlog SP3 datalogger has two gypsum sensors only separated by 5 cm, when checking the data both values were very similar thus we decided to calculate the mean value of the both gypsum sensor, i.e. at the end we have 1 water potential values per each Microlog SP3 and year. To avoid misleading data, we only use the water potential data during the growing season (from April to September, avoiding below 0ºC) in 2022 and 2023. Clarification has been made in the L 153- 161 and in the figure 3 caption.

As more loggers were added in June 2022 and are still collecting data currently, we have now downloaded the updated data we were able to include data points from all four summits in figure 3B.

Table 1 is somewhat confusing because it makes it seem like there were two different experiments with their own levels of replication and unique data -- one focusing on storage treatment, and a separate focusing on water potential. In reality, it seems like all water potentials were applied to all subpopulation x storage combinations to result in a single dataset… and these replicates contributed to the analysis of storage treatment, water potential, or both assessments (i.e. you did not have totally unique sets of replicates for different analyses).  I am wondering if you even need Table 1, or if you could just drop it – I think I had a clearer understanding of the study design and analysis from the manuscript text and Table 2 (which clearly shows how the treatments were applied across different subpopulations. You could also clarify in the data analysis section by simply stating the number of subpopulations/replicates that contributed to each analysis you describe.

We agree with the reviewers view that table 1 might not be necessary, nevertheless it was a Journal requirement specified in the submission guidelines. Therefore, we rely on the Editor's consideration to keep or leave Table 1 in the manuscript. We added the clarification of the number of subpopulations that contributed to each analysis in L 289- 294.

L297 -298 I assume you included all possible subpopulations and replicates for this analysis, whether data was available for only one or both storage conditions (fresh or AR)? Maybe you could clarify that. I’m also curious whether patterns hold if you reduce this analysis to only the six subpopulations for which fully-crossed data are available (i.e. both storage conditions and all water potentials)?  Have you distributed the storage treatments (fresh, AR or both) across subpopulations evenly such that there is unlikely to be bias based on which subpopulations received only a ‘fresh’ or ‘AR’ assignment (for the 12 receiving only one storage treatment)?

Yes, we included a clarification in in L 289-294.

As suggested by the reviewer we have repeated the analysis with the reduces dataset with fully-crossed data (subpopulations N= 6) and the model show concordant results with the previously reported using the full dataset. Seeds in the after-ripening storage treatment showed significantly higher germination than fresh seeds. Confirming a certain degree of dormancy in fresh seeds. Details of these results have been added in Table S3 in Supporting information.

We carefully distributed the storage treatments across subpopulations to include representation from all submits in both storage treatments. We clarified this distribution in L 291-293.   
  
L299 I had to do a little bit of digging to understand that you are estimating ‘median’ water potential as described by Bradford et al. (1990) rather than base water potential of any particular germination subpopulation (which some studies report using). Bradford’s approach is referenced in other parts of the paper, but it would be helpful to give details on the specific metric you are estimating here (i.e. median base water potential), where you are describing the details of the hydrotime analysis. Also, does the median base water potential represent an equivalent metric to the mean base water potential described by Bradford et al. (1990) as an estimate of base water potential for the 50th germination percentile? Why estimate the median instead of the mean?

We appreciate the detailed comment. There was a slight term confusion in the manuscript. We specifically estimated the base water potential using the estimate for the 50th germination percentile following Bradford et al. (1990). We have clarified the methods in L 302-303 adding the details of the metric used in our research.   
  
Table 2. The conceptual significance of sigma is never described in the paper, and the other parameters are not so easy to interpret (theta, R2) unless one has a background in hydrotime modeling or read Bradford very carefully. I think a bit more description of these parameters, even in the figure caption would be very helpful to readers (e.g., sigma represents the standard deviation of the base water potential estimate, no?). If it is possible to show the figures underlying the estimated parameters from Table 2, I think that would be even more helpful to interpret these metrics and how they are derived (particularly as I expect future studies use this paper and R package to estimate base water potential). I would suggest adding these to the supplement along with an explanatory caption that describes the estimated metrics.

We agree with the Reviewer's comment. We have added a description of the different parameters used in the caption of Table 2 L 774-780.

We agree with the reviewer and have included the figures underlying the estimated parameters from the models in Figure S2 in Supporting information.   
  
Table 2 Several estimates of base water potential are positive – why is that, and is that physically/ ecologically feasible? Bradford et al. 1990 describe 0MPa as the upper limit for base water potential, and as far as I can tell  from the supporting figures, all of the seedlots did reach at least ~50% germination at 0MPa. I expect that positive estimates could result from attempts to model median base water potential from observed data with relatively little germination (and few germination percentages near or above 50%)... But would it be more appropriate to assume a maximum base water potential of 0MPa, or to integrate a maximum base water potential of 0MPa into the modeling process in order to avoid positive estimates?

The positives values, even if surprising, are feasible when seed lots at 0 Mpa (with water) have below 50% germination. After carefully checking the data, we would like to point out that even though the mean germination shown in figure 1 of supporting information is indeed above 50% in all subpopulations, some of the replicates (i.e. petri dishes were in fact below 50% germination). We have added a clarification in L 320-323 and modified the Figure S1 in supporting information to include a ribbon with min and max germination to improve visualization and avoid misleading.

We agree with the reviewer point, and positive values are actually an artefact created when hydro-models are applied in populations with below 50% germination. We repeated the models keeping the maximum base water potential of 0 Mpa (clarification added in methods L 320-323) and the results did not change (more about model modification in the comments below.   
  
L301-303  In the text, can you briefly describe the process by which seedr models and returns base water potential? I have not used this package and am unsure whether there are options to choose different model types or specifications when estimating base water potential, or how it combines germination data from multiple replicates. Including brief details in the text would be helpful.

The package applies the exact Bradford et al (1990) theoretical model. Step by step stages have been described in lines 305-319). As synthesis, the package changes cumulative germination proportions to probit data, then it is transformed for each data point by multiplying the time (day of germination check) by the water potential treatment applied on each specific treatment. Specifically, it aggregate the germination data from each replicate (i.e. petri dish) and calculate the cumulative germination before applying the models.

There are no options to choose different model specifications to estimate base water potentials in the hydrotime models (although there are different options when considering thermal time), a more detailed explanation of the options is provided in the online repository where vignettes are also available. (https://github.com/efernandezpascual/seedr).

L308 I would spell this out – ‘We found a significant interaction between storage and GDD’

Done  
  
L308-301 Is it necessary to repeat separate, new models for each treatment? Consider using post-hoc comparisons for your full model that offer insight into significantly different treatment combinations (e.g., via the emmeans package in R). These can also be specified to adjust for the multiple comparisons you are making, which may be more appropriate for statistical power than simply rerunning additional, separate models with the same dataset.

Thank you for the suggestion. We have reconsidered the separate analysis, as we have only 2 levels of storage treatment, we actually don’t think is necessary to apply a post hoc. The significant estimate of the interaction already allows us to interpret that the relationship between ψb and GDD in fresh seeds was significantly different than in after-ripened seeds. The separate analysis allows us to test that relationship's strength in both storage treatments. We clarified the justification in lines L 328-334. Nevertheless, we repeated the analysis with maximum bWP values = 0 and the results remind equal.

L311 Dickman et al. 2019 (which is cited in the references) included seed mass as a covariate in all models to account for maternal effects. I wonder whether it is worth looking at a similar approach here, to understand not only whether base water potential varies by seed mass, but if seed mass alters or explains the relationship between base water potential and GDD?

Thank you for the comment, we did checked the relationship between seed mass and base water potential/GDD, but lastly, as the results showed not significant effects of seed mass we decided to not included in the final version of the manuscript. After careful consideration and detailed analysis, we agree that seed mass is might be an important covariate, event though we found no significant effect. thus we report its results in methods (L 341-343 and 346-349) as well as details of the model and figures in supplementary XXX??. Nevertheless when seed mass is considered as a covariate we found no signficatn effects of GDD in neither storage treatment.   
  
L340-342 Although it does appear that base water potential tends to be lower for after-ripened seeds, don’t the results of your model suggest that this is not a significant difference? (Table S4) If you formally tested this, the analysis results should be clearly stated here. However, I realize this text refers to patterns for the six subpopulations tested in both storage conditions. If you suspect that these results are different (something I was also wondering above, L297), then perhaps consider formalizing and reporting results from a reduced  analysis of storage effects for those six subpopulations.

We think there was a misconception, table S4 in after-ripened model, there is a significant effect of GDD on base water potential. Nevertheless, we partially agree with the reviewer comment and have redone the analysis only with the 6 subpopulations tested in both storage conditions. Results remain the same with a significant interaction between GDD and storage treatment and when analysed separately to test the strength of the relationship only in after ripened seed, higher GDD correlates with lower germination base water potential. Model results have been added in Table S4 in Supporting Information and clarification has been made in methods L 296-298 and L332-334.   
  
L401-405 Can you include some suggestions for how we could test alternative hypotheses, or refine mechanistic understanding, moving forward?

We have added some alternatives we considered to improve our knowledge, for example: to collect seeds from extreme base water potential plots, then do reciprocal sows and control germination in rainfall episodes of diverse intensity while measuring soil base water potential. This idea has been added in discussion L 487-491.   
  
L406-424 Maternal effects are not explicitly mentioned here, but I might discuss their potential importance too, and again, suggest reconsidering an analysis of whether and how they may play a role (by integrating seed mass a covariate in models).  
We checked seed mass covariant and added it in the models, we found no significant maternal effects in germination base water potential even though it did seem to interact with germination responses at intermediate levels of water stress. See methods clarification L 341-343 and 346-349, reported results in supplementary table/figure?? and discussion L 400-408.

L444-446 True, but an alternative approach is to sow seeds in the field and simulate/predict germination timing based on your estimated hydrotime parameters and measured soil water potential to understand how well your lab-estimated parameters and hydrotime models do at predicting field germination outcomes. I think that hydrothermal parameters are potentially very useful and important as species or population-level traits, yet a major limitation continues to be a lack of evidence that they contribute to meaningful differences in germination time in the field.  I think this represents a significant point for discussion (see also next comment).

We agree it would be very interesting to test this hypothesis in the field. We added to discussion L 474-476.   
  
Discussion point: Although there is a significant, negative relationship between GDD and base water potential for after-ripened seeds, the range of base water potentials range from -0.35 to -0.55 MPa, with 5% of values falling into the narrow range of -0.40 to -0.48Mpa. Is this range of variation likely to translate into meaningful differences in germination time in the field? I think the authors should clearly mention the observed range of variation in the discussion, at minimum, and if possible, try to make some interpretations about the likely impacts to germination dynamics at their site based on the observed microlimatic data that they have (Figure 3).

Thank you for the suggestion we think this is a very interesting and we have developed this idea. We have added a new measure to calculate, from our data in the field how much time it takes to go from -0.55 to -0.35 in two different time periods; from 1st of July and from 1st of August (the two months with more drought). Results showed very little (few hours) or non-existent time differences, meaning that once a rain episodes happens very rapidly surpass de germination base water potential range limits. These results suggest a limited ecological significance of base water potential in the field, even though this species has been proven to germinate within few hours in the lab. We added this discussion points in lines 425-433.  
  
L447 to 451 expand on points mentioned above, but L451-453 has less context. Can you offer more about whether and how base water potential for germination should be related to other aspects of seed regeneration? Was it related to dormancy in the current study?

We have expand the discussion in L 492-501. The differences to reach germination base water potential, thus the time when germination can start, might be influencing seedling development times, considered highly vulnerable, as well as rehydration cycles in the soil seed bank, which can modify seed soil persistence and viability.   
  
L454-461 I echo my comments from above with a reminder here that this study shows an important trend, but cannot directly demonstrate that base water potential influences fitness, success, or performance in the field, or that this level of observed variation translates to meaningful differences in germination timing that will influence outcomes in different climatic contexts. It is an important first step, and these are all future directions of study. So I might pare these statements back, or build clear support for them throughout the discussion with more direct evidence from other studies, if they exist.

We agree and have tone down the statements.   
  
Reviewer: 2  
  
Comments to the Corresponding author  
This manuscript details a highly important and understudied aspect of seed ecology research – the water thresholds for required for germination – linking field conditions with a laboratory study and using PEG to regulate the amount of water available for germinating seeds.

We appreciate the Reviewer 2 positive opinion about our research. We appreciate all the comments and suggestions, which have been incorporated in the manuscript and discussed in the comments down below. We think the new manuscript has improved through the reviewing process.  
  
The Introduction is well structured and written and out the study into context, including couching the topic in terms of its functional importance and referring to the theory behind hydro-time models. The hypothesis and rationale for the study is well developed, however I would have preferred some more specifically articulated aims for the study, to indicate the details of the various components of the study.

We agreed and have explicitly added the aim of the study in L121-122.   
  
The Methods are clear and suitable to test the hypotheses. Did you also consider whether cold-stratification would be useful to alleviate dormancy in this species – given that some seeds in the wild may persist under snow through winter and then germinate the following growing season?

Yes, we did considered it, nevertheless, we had previous knowledge available from a germination phenology experiments mimicking field temperatures which included the same species, now under review in another peer-reviewed journal. In this previous experiment, the focal species, germinate to 100% in autumn conditions between September and November, most of the seeds within the first two weeks without the need for cold stratification. Considering these results, we decided to test responses to water stress when apepars to be germination in the field. More clarification has been added in methods L 232-236.   
  
In the germination experiment at 20C with the PEG solutions, did condensation (water droplets) appear inside the petri dishes, and if so, how did you deal with this issue given that the you’re trying to control the amount of water available to the seeds? Also, please explain how you tested that the water potentials in the petri-dishes in this experiment to ensure they were accurate.

We appreciate the comment and added more details in methods L 261-266. Condensation did not happen due to the daily checks being done, nevertheless each petri dish was not open for more than a few seconds a day, so we assume a low effects of evaporation because the germination experiment only last 28 days and even if some evaporation happened we assume that the effect in all petri dishes will be similar. Our manuscript does not focuses on the absolute base water potential value found in the lab but rather in the important patterns underneath the germination base water potential and the differences observed at subpopulations level.   
  
Is there a way to visually represent the Bradford hydro-time model results? The patterns across the field collections (sub-populations) aren’t very clear in a table.

The pattern across subpopulations can be better observed in figure 5 nevertheless, we have also added the figures underlying the estimated parameters from the Bradford hydro-time models in Figure S2 in supporting information.

The Discussion section is insightful and again highlights the importance of the work in the context with other studies of the abiotic factors restricting alpine seed germination under natural conditions. The caveats of the study are also not unexpected and do not detract from the overall results of the study. Reminding readers to carefully translate laboratory results to field conditions is wise, given the natural variation that occurs in the field.

We appreciate the positive comment.

On line 450, change ‘sows’ to ‘sowing’

Done  
  
A really nice study. Well done

Thank you for all your insgihtfull comments.