## Reply to Reviewer 1:

Your evaluation was of particular and valuable importance to improve our manuscript. It is great to have reviewers like you, who spent a lot of time to carefully read through manuscripts and really help to put it onto the next level. Thank you. In the following we would like to comment on a point-to-point basis on the given reviewer comments (reviewer comments are given in bold, answers in given in italic).

**Does the paper present novel concepts, ideas, tools, or data?**

**Yes. This manuscript addresses an important gap in the scientific publication record on Submarine Groundwater Discharge (SGD). Specifically, while SGD has been widely studied over the last decade, almost all studies lack data that can define temporal and spatial scales of SGD precisely. The reviewed presented manuscript successfully aims at accomplishing this. The remote sensing data collected for this study is novel in is presentation and processing.**

*Thank you for your evaluation. We too think that there is a gap of a combined analysis of temporal and spatial scales of SGD and we hope our study is the onset for further studies that seek to accomplish to bridge that gap similar to what is done in the Near Shore Imaging community.*

**Are substantial conclusions reached and do the authors give proper credit to related work and clearly indicate their own new/original contribution?**

**Substantial conclusions are reach in the form of the methodology presented in this manuscript. A processing method is used that can lead to significant help for other studies in identifying SGD hotspots. That said, the authors fail to mention that the presented methodology, while new to thermal image processing, has been used by the Near Shore Imaging community such as the producers of Argus for a while. The authors should not only reference this work (e.g. Holman et al., 2017 and therein https://ieeexplore.ieee.org/document/7809056/) but also change the language of their manuscript to match the existing technical language used in the field.**

*To be completely honest, up to now, we were not aware of Argus, nor Cosmos, nor any other system, which uses optical camera solutions to monitor beach dynamics, long shore currents or derive near-shore bathymetry. The latter, or more specifically cBathy, by the way, is pretty interesting and could also work with thermal infrared data since wave crests are perfectly visible due to the changed incidence angle and thus the changed emissivity. The suggested work by Holman et al 2017 matches our method and approach in large parts, starting in the idea itself using a fixed camera (whether it is from an UAV platform or from a post), through the creation of a data cube after registering all images (whether it is with fixed GCPs as in the Holman et al. case or with an intensity based image registration system requiring no GCPs at all in the present case), up to the time series analysis of a continuous cross-shore*

*stack of pixels to illustrate a certain geometric variance. Thus, we will certainly cite Holman et al.*

*Concerning the technical language, we will adapt some terms (e.g. time stack, data cube, cross shore scale) mentioned throughout Holman et al. 2017 and earlier Holman publications to match technical language. If reviewer 1 thinks of further terms to be matched, we would be eager to know them and to discuss their integration as well.*

**Are the scientific methods and assumptions valid and clearly outlined?**

**The scientific methods are clearly described and valid. They could be better backed up by above mentioned references. A comparison for example between existing Image products in Argus and those presented in this manuscript would be little effort but very effective.**

*As suggested, we will back up our methods with references from the Near Shore imaging community. In this context, we will discuss Argus product (e.g. Time Series and Trend analysis) and our result/product.*

**Are the results sufficient to support the interpretations and conclusions?**

**The results on SGD hotspot identification and current movement are sufficiently supported and deserve publication. That said, I find the conclusion on effects of geology on discharge pulses rather speculative.**

*Due to missing tomographic investigations of the related geological formation (the dry-fallen lakebed), we have to speculate, that geological effects may force the observed discharge pulses. We propose, due to a randomly developed network of karst structures, discharge effects such as known from pulsating springs/gushers are observed. However, we emphasized in the manuscript that we only <u>propose</u> the maturity of the karst system to be <u>one possible</u> explanation. To be even clearer on that issue we will rework the concluding statement.*

**There are three potentially minor problems with the conclusion of the manuscript: One: The authors should address the potential problem with utilizing only two reference reflectors (see Fig 1). A plane cannot be established with only two reference points, thereby potentially underestimating the distortion effect a changing plane can have on the recorded pixels. See above mentioned reference. This can easily be incorporated into the manuscript by including a section on "limitations and potential errors", which is already mostly written.**

*The method we used for co-registration of the slave image onto the master image does not depend on the two reflectors only, but on all rigid land parts (several hundreds of pixels), or more specifically on the similarity of the intensities of both images. Since the rigid land parts are similar in intensity in master and slave images, the rigid land parts will be the only image part taken into account*

*In a first step, the similarity matrix between the two images is calculated. Incorporating scaling, rotation and translation, in a second step, the slave image is iteratively transformed and each time compared to the master image using the similarity between the master and the transformed slave image. The similarity "goodness" is evaluated each time using a mean square metric (which we chose to be a regular step gradient descent) as accuracy measure. As long as neither the maximum iteration criterion is reached (in our case 1000 iterations), nor the quality criterion (in our case a Maximum Step Length of 1.0e-2) the optimization process is continued until one of the two criteria is reached.*

*This process is repeated for all slave images. To provide a further independent measure accuracy measure we used the two reflectors. Similar to the automatic approach describe in Holman et al. 2017 to find GPS targets, we defined the search windows in the co-registered images looking for the lowest radiance values (=reflector plates). Since the plates represent an area of several connected pixel we then extracted the mass centre of both plates and compared the coordinates of the mass centre to the coordinates of the mass centre (reflector plates) of the master image, getting the spatial accuracy among images (Fig. S1), giving us a criterion on how to evaluate spatiotemporal changes in the light of image registration uncertainties. Thus, we are convinced distortion effects due to the image registration process to be minor in the presented case. For reproducing purposes however, as suggested, we will add, a section "Limitations and Potential Errors" picking up possible distortion effect with less land parts, and other possible pitfalls hindering a successful reproduction of the proposed method.*

---

**Two: The authors must include a discussion of bathymetry and it's potential effect on the data in a more comprehensive way. Is the substrate flat etc.**

*We added a more comprehensive discussion concerning the bathymetry in the revised version of the manuscript.*

---

**Three: In section 4.3 the authors write "In this context the question arises on the transferability of the presented approach" but they don't give a qualitative answer to this question. A short discussion on how the results may vary in SGD studies that don't have the extreme buoyancy differences should be included.**

*We agree and will elaborate on transferability, possibly within the newly created section "Limitations and Potential Errors".*

---

**Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?**

**Yes.**

---

**Does the title clearly reflect the contents of the paper?**

**Yes, but it is a bit technical.**

*If the reviewer doesn't mind, we would keep the title as it is.*

---

**Does the abstract provide a concise and complete summary?**

**Yes.**

---

**Is the overall presentation well structured and clear?**

**The overall structure is clear and follows a clear structure. That said, there is a significant typo in the manuscript. What is labeled as Methods is actually Results.**

*Thank you and yes we agree, there is a significant typo that is already corrected. After having been pinpointed to this typo we encountered further numbering mistakes in headings (e.g. 4.3 occurs twice) and figures. All of them are already corrected, but will be double-checked again before submitting the revised version.*

---

**Is the language fluent and precise?**

**NO!! There are significant problems with the language. The manuscript is speckled with syntax and grammatical mistakes. While I have the utmost level of sympathy for this issue the authors should utilize the help of a professional editor. There are a number of logical problems in this manuscript that I believe are a result of the significant language problems displayed in this manuscript. Also, there are many subjective or judgmental adjectives in the manuscript (i.e. the use of the word "clear" or "clearly" is used 20 times alone in the manuscript). All of them should be eliminated. All this being said, in light of the very high value of the content of this manuscript I still consider this problem to be minor and easily fixed. I wanted to assist in this task and have attached a PDF where I have marked up some of the most obvious mistakes.**

*We are infinitely grateful for the very detailed improvements reviewer 1 gave in his/her attachment. This sort of assistance, that most certainly demanded an enormous amount of time, can no one expect and is rarely offered to push the overall value of the submitted manuscript beyond the content-based value. Again, we really appreciate the efforts of reviewer 1, will of course incorporate his/her suggestions, and will also utilize the help of a professional editor.*

---

**Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?**

**For the most part yes. There is an inconsistency in the use of the symbol "~". For example it says on Page 7 Section 2 (which is actually Section 3) "~20 to ~46 pixels (2.6m-6.0). If the pixels is inexact then the measure of meters must be to. The same applies for the rest of the manuscript. Consistency is lacking in the use of abbreviated unites and spelled out unites, i.e. seconds and m.**

*True, we will rework the consistency aspect.*

**Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?**

**Yes, large sections of the Results should be summarized in Tables. This would make a comparison of SGD sites much easier for the reader. The descriptive nature of this portion of the paper is very tedious to read. Also, section 2.5 "Water Chemistry" is a major constituent of this study yet it only pops up in the results section. This section should be expanded and better introduced in the beginning of the manuscript. It is not clear to me how section 3.1 or Fig. 5 or 6. explain the "conditional nature" of each spot. Please expand and explain more clearly.**

*We agree with the reviewer and expanded the section of water chemistry including an adequate introduction to make it clearer. We also added a table summarizing the results to gain more clarity. Concerning the "conditional nature", we believe we may have evoked a misunderstanding. The term "conditionally" in section 3.1 (p8L21) refers to the variance analysis and its limited expressiveness concerning specific temporal behaviour. The way the variance analysis is performed and presented provides a general view (over the entire analysed time period or the entire time stack) on temporal behaviour. Yet, the variance analysis prohibits insights in specific parts of the temporal period being analysed, say only the first half or even single temporal elements such as single images as in the presented case. This is why we wrote "conditional", as it cannot shed light on specific parts of the analysed period. To avoid confusion, we changed the sentence accordingly.*

**Are the number and quality of references appropriate?**

**The presented references are fine but a large body of work by the above mentioned reference should be included. On page 3 section 2.1 the authors mention that the site this study was done at was the site previously studied by others. It is important to summarize these previous studies and put your study in their context. Simply referencing does not suffice in this case.**

*We changed the passage and tried to put our study in a proper context.*

**Is the amount and quality of supplementary material appropriate?**

**Yes.**

**AGAIN, I want to make clear that I find this manuscript extremely valuable despite some significant language and syntax problems.**

*Thank you, we really appreciate your evaluation and want to thank you again for your efforts, time and passion spent to help us improve our manuscript.*