Answer to Reviewer 1 regarding Bello et al. paper submitted to *Drones* journal:

Thank you very much for your many suggestions to improve our manuscript. All of your questions and suggestions have been taken into account in our revised version, as described below. We have kept your comments in black and included our answers/comments in blue.

Comments and Suggestions for Authors

Although the work appears to be very interesting, in many places its descriptions are not timely and misleading. I agree that conducting drone surveys in the Antarctic is very difficult due to the very harsh weather conditions, particularly in the South Shetland region, and for this reason, any further work that tries to address this problem is valuable, but the one I reviewed needs a lot of tweaking to reach a certain required level for a Remote Sensing journal. My concerns are detailed below

Thank you again for your thorough review. We hope that the revised manuscript will satisfy your expectations. In fact, we have made so many changes that we have not coloured them, as nearly all of the text would be coloured, not helping to an easy reading.

Already the first sentence in the abstract is questionable. The authors ask a question: "What type of drone is most suitable for surveying the polar glaciers of Antarctica?", and in chapter 4 they answer affirmatively as follows: "the first hypothesis raised, in relation with the type of unmanned aerial system most suitable for flying in the study area, was confirmed" but in the whole paper I have not found, not a single result that supports their thesis. In my opinion, such a claim would require the use of at least several drones (e.g., those commonly used in the Antarctic produced by e.g., DJI), of different sizes, using them under different conditions and then statistically analysing the results. This is what I do not find in this work.

You are right. The research question posed was inappropriate, as no intercomparison of available systems was performed in our paper. We have changed the main research question to "How do the weather conditions typical of the polar maritime glaciers of the western Antarctic Peninsula region affect to the flight operations of fixed-wing drones and how should these be adapted for a successful flight?", which fits much better the aims and contents of our paper. In fact, it was not our intention to make an intercomparison between fixed-wing and multirotor drones. We have clarified this right in the introduction (see lines 61-62; "We wish to emphasize that our study does not aim to be a comparison of fixed-wing versus multi-rotor systems. The latter have also been shown to provide excellent results in the Antarctic environment (e.g. [15]).").

Moreover, the introduction needs to be substantially revised as it is not up-to-date and, worst of all, may mislead readers e.g.

The second paragraph suggestively points out that there are no such works that use the fixed-wing UAV is rare in Antarctica and there is any word about the previous success obtained by other teams with use of fixed-wing UAV i.e.

Application of UAV BVLOS remote sensing data for multi-faceted analysis of Antarctic ecosystem by A Zmarz, M Rodzewicz, M Dąbski, I Karsznia, M Koncak-Abshire, ... Remote Sensing of Environment 217, 375-388

You are right. We have now clearly acknowledged the existence of other, previous uses of fixed-wing RPAS in the same region, and have given, as an example, the reference provided by the reviewer (see lines 63-65: "Neither we pretend that our study is pioneer in the use of fixed-wing RPAS in environments such as the South Shetland Islands, as there exist some notable examples (e.g. [30]).").

In line 36 the authors claimed: "and were not focused on the modelling of the glacier surface", which is not true, as for example

The Influence of Shadow Effects on the Spectral Characteristics of Glacial Meltwater by KA Wójcik-Długoborska, RJ Bialik Remote Sensing 13 (1), 36

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in only one paper presented results for 6 glaciers, which were obtained using DJI Inspire 2 (multirotor). The authors in this paper made raids from a height of 500 m and the area size reached 1.5 km2. This should be changed.

Again, you are right. We have modified the mentioned sentence to "they were not often focused on the geometric modeling of the glacier surface, though some notable applications exist [15]." (lines 39-40), citing -as an example- the reference given by the reviewer. In any case, we wish to emphasize that we tried to distinguish (when mentioning some examples of applications) the research done before our field campaign (2014-2015) from that done afterwards. We have now added some further references published "after" our field campaign. The motivation for publishing this article now, past several years since the data was collected and processed, is that we have received from other colleagues or research groups inquiries about our experience in the South Shetland Islands environment, and hence the practical guide appearance/format of our paper.

With reference to the above comment, what the authors wrote in lines 52/53 is also questionable, as today battery life is not an issue, as one mission, e.g., using PIX4D software, can be performed on multiple battery packs that are changed during the mission. This all needs to be clarified and detailed, as it is simply wrong.

You are right regarding the many advances in this matter (in particular, the use of battery packs). However, we have tried to stress in the revised manuscript why battery life is still an issue when working in environments such as ours. See e.g. lines 122-129 of the revised manuscript. Also, please keep in mind that our fieldwork was done in 2014-2015, when not so many of the recent improvements regarding battery life were available.

The weakest part of the work is the results section, in particular section 3.1. In my opinion, this section is the conclusions, because I do not see any results there. However, even if one treats this chapter as conclusions, it scores points:

We have provided in the results section further data/tables/figures/explanations supporting our statements, as detailed below.

1. have not been proven in the work.

An example of weather forecast extracted from AEMET and ECMWF for Livingston Island has been included (Fig. 4) , as well as a summary table (Table 6) with the daily weather conditions during the period of the flight operations. It is discussed in the text (Section 2.4) how such weather conditions influenced the various flights. We have also added a link to a video illustrating the windy conditions at the study site.

2. it is not stated in any case what the area of the raid was.

A table has been included (Table 7) comparing the coverage with full and reduced battery level. With such information, the distances that the drone could fly were calculated and the safe takeoff point established. These calculations were made using the flight planning software (Trimble Access). A related figure (Fig. 10) has been introduced. Also, a complete map of the areas flown has been added, as suggested (Fig. 3).

3. this work does not prove how atmospheric conditions affect the material of the drone.

Various elements (including videos, and explanatory texts) have been added to exemplify the influence of the atmospheric conditions. For instance, on the tension of the shuttle tires necessary for takeoff, or how an increase in tension is given to the loop to compensate for the lack of elasticity due to the cold.

4. as above. No results proving this record.

Unfortunately, the moment when the plane fell due to the freezing of the Pitot tube because of a combination of fog and low temperatures was not recorded. Though we have added a video of the first flight where the logistic difficulties of accessing the work zone, and the fogy weather conditions can be appreciated. We have also included an image (Fig. 12), provided by NASA Spinoff, belonging to a study carried out at the Glenn Research Center. They use different tools such as the Center's Icing Research Tunnel and the NASA 607 icing research aircraft, as a "flying laboratory" for studying icing conditions. With these investigations they have shown that the accumulation of ice on aerodynamic surfaces can drastically affect the aerodynamics of an aircraft. In our particular case, the record of the drone’s black box was sent to the manufacturer to establish the reasons for the fall and they returned a report indicating that the plane suffered this sudden fall due to the freezing of the Pitot tube because to the low temperatures during the flight (this later explanation has been added to the text).

5. as above. No results to support this conclusion.

Regarding the study of snow reflectivity and its effect on image processing and point cloud generation, we have included images before and after the processing/filtering (Fig. 13). In this particular case, the image shows the point cloud of JCI station, first without filtering or classification and second after the entire process described in section 2.6 was carried out.

6. No information on mission coverage with photos.

As mentioned earlier, a map of the mission coverage has been added to the revised manuscript (Fig. 3). Also, in the results section the images of the map of the unexplored flight zone U3 and of the controller during the operation of flight 13 have been incorporated (Fig. 14). The latter corresponds to the event of loss of GPS signal and subsequent communication failure with the aircraft occurred, which led to the emergency operation of returning the plane to the takeoff/landing area.

Finally, a review of the literature cannot be limited to the claim that a great deal has been described in the work of Pina and Vieira [17]. The authors should put in more work because a lot has already been done on this topic but has been omitted in this paper.

As mentioned earlier, we have added some new references, not all of them included in Pina and Vieira paper. Nevertheless, our aim is not making a literature review, and the brief one that we included in the introduction (and elsewhere) was just aimed to illustrate certain areas of application or to support some of our arguments.

I can see some new findings that the authors have gained and from this point of view I think they should be given a chance to improve their manuscript and although I am very critical, I am eager to read the revised version believing that the authors will be able to improve their manuscript.

Thanks for giving us a chance to improve the paper. We hope that the new version will be sufficient to satisfy you and the expectations for a paper in the *Drone* journal.

But please treat my comments as a guideline that represents 30% of the work that it is up to you to do and add 70% from yourself, in particular to ensure that your work complements what has already been achieved with drones in Antarctica.

This is what we tried to do when preparing the revised version, and hope to have succeeded.

The authors, 3 Nov 2022.