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Geoscientific Instrumentation Methods and Data Systems Discussions

Interactive comment on "An automatic DI-flux at the Livingston Island Geomagnetic Observatory, Antarctica: requirements and lessons learnt" *by* Santiago Marsal et al.

Santiago Marsal et al.

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Response to reviewer #2

Find below the original reviewer's comments followed by our answers. I have also attached a modified version of the maniscript as a .pdf supplement, according to both the reviewer #1 and reviewer #2 comments:

Page 2 line 7, "Variometric" is not a recognised term in English and either "variation magnetometers" or "variometers" would be preferred, although a GEM Systems proton magnetometer dos not fit either of these categories.

We have replaced "variometric magnetometers" with "variometers". We have also reorganized the succeeding text where we referred to the proton magnetometer as a variometer.

Page 2 line 13, the stable reference frame referred to here is the geographic reference frame and should be stated as such.

Done

Page 2 line 16, the authors may wish to expand on what is meant by "baseline evolution" here to underline the problem to be solved i.e. what are the causes of variometer baseline evolution, what are typical signals (period, amplitude & resolution) requiring modelling by the baselines, why is a linear interpolation between occupations inadequate and what are the sampling recommendations of the international community?

Done. We have expanded moderately our explanation on some aspects of our baseline at LIV, though we do not think we should go into much more detail here, mainly because this is not the main topic of the article and because we are in the Introduction section.

Page 2 line 28, reference Rasson et Gonsette, 2016 here for the GyroDIF.

Done

Page2 line 31, "In-house testing" is ambiguous. Does this refer to testing by the authors or a specification provided by the manufacturer? Also, which instrument does this refer to and is this consistent with the declination uncertainty figures in Section 2?

Even if we have preserved this sentence according to the original article by Hrvoic and Newitt (2011), we have explained what it means. In fact, this is a specification by the manufacturers referring to the AutoDIF. Also, the referee is right with the observation that the declination uncertainty stated here should be consistent with that given in Section 2. Indeed, both figures are not fully consistent, though they are reasonably close together: in this section we talk about 0.1', while in Section 2 we talk about "below 0.3' ". To avoid confusion we have rearranged the numeric figures somewhat:

note that 0.1' is the value given by the manufacturers in optimal conditions, though in the same article by Hrvoic and Newitt (2011) it is mentioned that some tests showed that the real accuracy was 0.2'. On the other hand, the value of < 0.3' stated in Section 2 was theoretically estimated by myself (the main author) on the basis of some coarse numbers (note that we are interested in an estimation of the uncertainty rather than a precise value of it), so we have replaced the sentence "below 0.3' " with "typically 0.2' ". We have also modified Table 1 accordingly. The conclusion is that the current AutoDIF accuracy is more or less 0.2', depending on the particular conditions of each observatory.

Page 3 line 9, do the authors have a reference for the assertion that the AutoDIF declination uncertainty is less than 0.3'? Similarly for the GyroDIF value of 3.6' stated later in the paragraph and in Table 1?

The < 0.3' figure was roughly estimated by the main author of the present article by considering the distance between the AutoDIF and its laser reflector in a preliminary project proposal where we were considering the most suitable option to us: either the AutoDIF or the GyroDIF. As for the stated GyroDIF value of 3.6', it is also an estimation by myself, but in this case the calculation is much more complex and would deserve a thorough theoretical development to justify it. It accounts for the noise of the gyro output signal and the particular procedure of the true-north angle measurement. Unfortunately, we can't give a reference for these figures, though we have tried to give some clues in the text.

Page 4 line 26, it would be helpful to state here something about the driving current for the heating system as it is assumed that this must be safe and have no effect on the magnetometer enclosed within it.

Done

Page 8 line 7, σt should read σf

Right. Done.

Suggested grammatical corrections: Page 1 line 29, "..as it is at our partially manned station.."

Done

Page 2 line 23, "The one with longer history is called AutoDIF . . . designed to reproduce its manual measurement sequence", would read better as, "The one with the longest history is the AutoDIF . . . designed to reproduce the manual measurement sequence of the DI-flux."

Great. Done.

Page 4 line 12, "preserving it in the system batteries in prevention of periods of scarce wind generation", would be better phrased as, "enabling its operation from system batteries during extended periods without wind-generated power."

Done

Further notes: Page 5 line 1, "basically" would be better replaced with "mostly". Note: heat loss by air exchange, which is easily estimated, can also be a lesser but significant factor as it is difficult to construct an air-tight enclosure. This may account for some or most of Pa. Some air exchange may be desirable e.g. to prevent the accumulation of damp.

Done. The referee is right with his observation that Pa partly accounts for heat loss by air exchange. However, we prefer to minimize it (and ideally prevent it) because the air humidity at LIV is usually very high, so new air coming in would imply more damp. Instead, we opted for an enclosure as airtight as possible containing a desiccant agent.

Please also note the supplement to this comment:
http://www.geosci-instrum-method-data-syst-discuss.net/gi-2017-22/gi-2017-22-AC2-supplement.pdf

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