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Subject Revision of manuscript 367

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Dear editor & reviewers,

Please find enclosed our revision of manuscript 367, titled ”Ocean Bottom Seismometer Clock Correction using Ambient Seismic Noise”. We thank you for carefully reading the manuscript. Just as carefully, we have revised the manuscript. We apologize for the delay. We conducted additional experiments and explored different avenues to independently recover the OBS clock drift rates (which turned out to be not straightforward). We believe we have now addressed all points that were raised. In addition, we have split the 'Results and Discussion' section into two separate sections. One paragraph of this section was moved to the end of subsection 3.4 because we deemed it more appropriate there.

Please find below the point-by-point response. We have uploaded two new files. The first is a revised version of the manuscript, and the second is the same revised version with the corrections highlighted in boldface. In the latter document, we have referred to the editor as **ED**, reviewer A as **RA**, and reviewer B as **RB**. We tried to find a compromise when the opinions of the two reviewers differed, or otherwise explained why it would be more appropriate to follow one or the other.

## Editor

1. The main concern raised by both reviewers involves the consideration of errors. This issue needs to be addressed, and a figure representing the errors needs to be added. (**ED-1**)

   *Thank you for the constructive feedback. We have revised our manuscript to address the reviewers' concerns. Specifically, we corrected the convention used for measured skew, clarified drift rate behavior, rectified an oversight concerning the leap second, reassessed our statements on skew accuracy, and introduced new figures to better visualize error differences. In addition, an in-depth discussion of the accuracy and precision of the recovered drift rates is added to the discussion section, as well as a detailed discussion of the (rela-*

*tively small) differences between the skew-derived drift rates and the drift rates recovered with our approach. We believe these revisions have significantly enhanced the clarity and robustness of our study.*

2. Please check that the revised version of your manuscript recognises the reviewers' work in the Acknowledgements section. (**ED-2**)

   *We certainly appreciate the importance of such contributions, as some of the coauthors also serve as editors and/or frequently review papers. We added a statement in the Acknowledgements section of our manuscript to recognize the (valuable) contributions of the reviewers.*

## Reviewer A

This article presents a new way to calculate OBS clock drift using noise cross-correlation, notably allowing one to calculate the "zero-time" clock offset. It also proposes an publicly available software allowing others to easily apply the same corrections. The theoretical and methodological explanations are detailed and clear. I am not qualified to comment on the deepest details of this theory/methodology, but it seems logical and consistent.

### Major problems

1. The first major problem is the apparently complete incoherence between the calculated timing error and the "Observed Skew". The difference between the two is between 0.37 and 0.96 seconds (assuming that the definitions are such that "timing error" should equal -"skew""). Moreover, they are not centered around zero, but always in the same direction. The authors' state in section 4.3 that "skew is not accurate enough" but that makes no sense to me because skew measurements are based on the instrument and GNSS clocks and should be precise to within 0.001 seconds. Maybe the skew measurements are completely inaccurate because of some bug in the process, but I can't see why they wouldn't be "accurate enough". As the differences are always in the same direction, I wonder if there might be another systematic error source. Did the skew calculations account for the leap second on 30 June 2015? Looking up the IMAGE experiment on the FDSN website, it looks like about half of the OBSs were deployed during that leapsecond, so not accounting for it would give a systematic difference which could go some way towards explaining the observed offsets, though admittedly not all the way, as there don't seem to be "1 second" and "0 second" offsets which would correspond to the instruments recovered after and before 30 June midnight, respectively. In any case, the leapsecond problem should be accounted for in the article. And Table 4 should include a column showing the difference between the skew and "timing error" for each station (**RA-1**)

   *We thank the reviewer for the meticulous evaluation of our manuscript and for highlighting the inconsistency between the estimated drift rates and the skew* *that was measured while recovering the OBSs. Since this first comment by Reviewer A includes several questions, we address these one by one.*

*First, as the reviewer correctly assumed, the convention that was used for the measured skew was opposite to our definition of $\delta t_i^{(\mathrm{ins})}$. In our study, we adopted the convention that negative values of $\delta t_i^{(\mathrm{ins})}$ imply that the recordings by station $i$ are subject to a time delay. We have now addressed this inconsistency by using the same convention for the skew. This has been corrected in the revised manuscript in Table 1, where we multiplied the skew by '-1' to comply with our definition.*

*Second, the reviewer notes that the measured skews (as well as our estimates of the clock error at the time of recovery; see Table 1) are not centered around zero. This is correct and often observed (Hannemann et al., 2014; Häble et al., 2018; Zhang et al., 2023). It can be explained by a drift rate that is consistently positive or negative for all stations. Note that the drift rate is dictated by the frequency of the quartz oscillators in the seismic clocks (Shariat-Panahi et al., 2009). The OBSs, which are part of DEPAS (German Instrument Pool for Amphibian Seismology), are SEASCAN microcomputer compensated crystal oscillators, which are temperature compensated.*

*Third, we are grateful to the reviewer for pointing out the oversight regarding the leap second. This was an oversight from the first author (DN) when reading the instruments' documentation. The skew shared with us was indeed not corrected for this leap second. Consequently, the skews are one second smaller than those listed in the reviewed manuscript. This applies to all OBSs, because all OBSs were recovered at the end of August 2015 (even though many of them at that point didn't store any data anymore due to a full disk). We subtracted $1$ s from the latter skews. Note that, in combination with the convention-related multiplication by '-1', this implies that the values that are now listed in the last column of Table 1 coincide with the initially reported skews minus one second and subsequently multiplied by '-1'. Finally, it is important to mention that all our time-averaged time-lapse cross-correlations (i.e., the $C_{i,j}\left(t, t_k^{(\mathrm{lps})}\right)$) are computed from hourly cross-correlations prior to June 30th. This implies that the "automatic correction" of the leap second to the land stations.*

*Fourth, stating that the "skew is not accurate enough" is indeed rather bold. The fact that the measured skews deviate from the drift values we estimated using our approach needs an explanation. We spent considerable time looking into this and have discussed this in detail in the discussion section (The assumption that OBS clock drift is linear may be part of the explanation.).*

*Fifth, regarding the additional column in Table 1 (we assume the reviewer meant Table 1 instead of 4): we have now included a new Figure (Figure 10) that nicely visualizes the difference between the skew-derived drift rates and the clock drift rates (the $a_i$) estimated using our approach.*

2. The second major problem is the lack of a figure allowing the reader to evaluate the accuracy and precision of the clock drift and incurred timing error estimates. Such a figure would allow us to visualize: 1) The time offset between cross-correlation measurements 2) The validity of the linearity assumption 3) How significant/clear is "Incurred timing error at t=0" The most obvious figure of this type would be an overlay of the linear fit with the cross-correlation offset

measurements.(**RA-2**)

*We acknowledge the reviewer's suggestion for clearer visualization. We added a new Figure 12, which compares linear corrections from our code, termed "OCloC-drift", with the skew values, referred to as "skew-drift". It also includes the time offsets between cross-correlations and a reference cross-correlation (RCF). In addition, an in-depth discussion of the accuracy and precision of the recovered drift rates is added to the discussion section, as well as a detailed discussion of the (relatively small) differences between the skew-derived drift rates and the drift rates recovered with our approach.*

## Minor points

3. Language: There are too many "filler" words/phrases which do not add to the information. Examples include "therefore"s and "that is"s (see line 88, for example) and "It should be understood that" (see lines 154 and 165, for example). The text would be shorter and clearer without them (**RA-3**).

   *We have minimized the use of such constructions.*

4. cross-correlation: is not a word, as far as I know. I've seen it written "cross correlation" or "cross-correlation", most commonly the second.

   *We changed it to cross-correlation(s)*

5. In the abstract, the authors should make clear what is new in their approach. As it stands, they present the OBS timing problem and then write "To address this issue, here we introduce a new method to synchronize the clocks of large-scale OBS deployments", which suggests that they are the first ones to use a cross-correlation method for this purpose (which I know they aren't trying to do, as they are very clear about this later on). I suggest they complete the "problem" part by saying that cross-correlation methods have been used for this, but they lacked "X". Then the "solution" part of the abstract could focus on what is new with the proposed method.(**RA-5**)

   *We have revised the abstract to highlight the novelty of our approach, and we have carefully set it in the context of existing cross-correlation methods for clock synchronization in ocean-bottom seismometers (OBSs). We now explain more explicitly that our method improves upon these existing methods by accounting for non-uniform surface wave illumination patterns and enabling the detection and correction of both linear and non-linear (sudden) clock drift during OBS deployment. We believe these changes helped emphasize our approach's unique features and its advancement in the field.*

6. Line 46: "these sensors": an OBS is not a sensor, it is an instrumentation that records data from sensors **RA-6.**

   *Thank you for bringing this oversight to our attention. We made it clear that we refer to the sensors within the instrument where possible. In other cases, we have replaced 'sensor' with 'instrument'*

7. line 206: "'timing errors' and 'clock errors' are used interchangeably.". Why? If they are the same, just use one of the two. If they are different, explain how and state that they refer to the same measure in the context of this article.

   *Using one of the two is indeed more consistent. We have now consistently adopted 'clock error' in the manuscript.*
   *(The use of 'timing error' stems from the 2021 GJI paper [Weemstra et al., 2021]. In that study, we allowed timing errors to be frequency-dependent because, in that study, the origin of some of the timing errors was absent/undefined transfer functions for some other (non-IMAGE) land stations. Consequently, the recovered timing errors turned out to be frequency-dependent. In the current study, we focused on OBSs only and didn't include those non-IMAGE land stations. The OBSs merely suffer from frequency-independent timing errors, which effectively are the same as clock errors).*

8. line 250-251: How do you reconcile this assumption of linearity with the results of Goeudard et al. (2014, Figure 11) and the jumps seen by Hable et al. (2018, Figure 8)? The new figure requested above should help to clear this up.(**RA-8**)

   *We appreciate the reviewer's observation. We recognize that the assumption of linearity might not always be true. Although the temperature affects the quartz oscillators' rate, the OBS clocks are temperature-compensated. We addressed this in the Discussion.*

## Reviewer B

While this paper is solid, logical, detailed, and was a joy to read, it appears to be an incremental improvement over Weemstra et al. 2021. On the plus side, new clock drift corrections are presented for the OBSs in the IMAGE network, which seems to be a useful contribution to the scientific community. Previous publications have used ambient noise cross-correlations to estimate clock drift, but have relied on the assumption that an instrument has accurate timing at the initiation of deployment. The current manuscript does not rely on that assumption.

1. The abstract and theory sections recognize that symmetry of ambient noise cross-correlation is a useful theoretical concept that is rarely realized in practice, even for land deployments. The introduction does not acknowledge this but likely should, at least briefly, mention it as well, given the errors that can be introduced by asymmetry. Example: Even the cross-correlations in Fig 8b (clock drift corrected cross-correlations) are not symmetric. (**RB-1**)

   *We deeply appreciate Reviewer B's thorough evaluation of our manuscript and the positive remarks. It is good that this omission was pointed out. We modified the Introduction section to emphasize the challenges when using seismic interferometry for correcting clock errors. We also modified the order in which the challenges are presented to fit with the same order as they are presented in the Discussion section. This addition highlights a key feature of the paper: the use of the weighted least-squares inversion.*

2. I am glad the authors recognize the existence of timing errors associated with suboptimal ray path illumination and spurious signals, but I am a bit concerned about how they are ignored in the inversion for timing errors related to clock drift. In a large data set, it is fair to ignore them if they are normally distributed. However, the $\delta_t(src)$ is not likely to be normally distributed, as suboptimal illumination typically leads to underestimated (rather than overestimated) travel times. If the data set is large enough, the authors could consider inverting for these terms as well (**RB-2**).

   *This is an interesting remark. In fact, in our previous study, we addressed exactly this, as it was also one of our concerns. In that study, we separate the effect of the azimuthal variation of the noise illumination from the effect of the station-station separation on the surface wave arrival times. That is, we decompose* $\mathbf{n}^{(\mathrm{src})}$ *(which contains the* $\delta t_{i,j,k}^{(+,\mathrm{src})} + \delta t_{i,j,k}^{(-,\mathrm{src})}$*). (For details, please see page 1042 of Weemstra et al., 2021, starting just before equation (18).) We subsequently include the mean of (potential) surface wave travel-time errors (resulting from a non-uniform illumination) in the inversion. This results in an extended weighted least-squares estimator of* $\mathbf{t}^{(\mathrm{ins})}$ *(equation (26) in Weemstra et al.). For some (arbitrary) non-uniform illumination patterns, however, it turned out that this did not yield more accurate timing errors than the weighted-least squares solution in which we did not account for (potential) deviations from zero of the mean of the illumination-related travel time errors. (See Figure 10 in Weemstra et al.)*

   *We expect that (part of the) the explanation for this is the SNR threshold that is imposed. This SNR threshold may effectively filter out most of the lapse cross-correlations that contain interferometric surface waves that arrive too early. In particular since noise sources that are not located in one of the stationary phase directions (which generate the very signal that leads to underestimated travel times) have a detrimental effect on the SNRs (Boschi & Weemstra, 2015). Given the results in Weemstra et al. (2021), we chose not to compute the extended weighted least-squares estimator in equation (26) of that study, but merely the weighted least-squares estimator that weights the data points based on the station-station separations (equation 16 in Weemstra et al., 2021).*

   *Finally, we added a discussion of this in Section 5.3 of the Discussion.*

3. Table 1: This is great information that needs to stay. However, in addition, please visualize the information in a graph, with the x-axis being the deployment period and y being measured (at the end (skew)) and inferred timing errors and their evolution during the deployment along with confidence regions for all stations.

   *We appreciate the recommendation. We've added a figure illustrating the deployment period against both measured (skew) and inferred timing errors, including confidence regions for three stations (Figure 12), and for all stations in Appendix C*

4. l.44-45: Two examples are offered for the use of OBS deployments. One of these is the identification of asthenospheric flow (Barruol et al., 2019). This does not seem like an appropriate example for a paper on clock errors as this type of OBS usage does not depend on accurate timing. Can you find another example(s)?

   *This is a good suggestion. We changed the reference to Tary et al., (2011) for experiments showing novel results and added studies with common uses of OBSs.*

5. l.47 It is recognized that atomic clocks are more accurate but too costly. In this context, please state what type of clock is actually being used in OBSs (quartz oscillators?). (**RB-5**)

   *Yes. Specifically, SEASCAN microcomputer compensated crystal oscillators, which are supposed to be temperature-compensated, are used. We have now added this in both Section 3 ("Implementation& application to Data") and the Discussion.*

6. l.62 and 66: "times" and "time" (and likely elsewhere in the paper). I propose to replace these by "time lag(s)", for clarity.

   *We replaced "times" and "time" with "time lag(s).*

7. l.116-117: A 1984 paper is one of two papers cited for "establishing SI theory over the last two decades". However, 1984 was four decades ago.(**RB-7**)

   *The reference was substituted with a reference to Lobkis and Weaver (2001) and Snieder (2004).*

8. l.279-280: The inverse problem (without a land station with known timing) is rank deficient by 2. It would be good if the authors can provide insight or a brief explanation, even if intuitive, on why that is to be expected.(**RB-8**)

   *We have added a brief, more intuitive explanation. In addition, we explicitly referred to Weemstra et al. (2021; Appendix A), which contains a detailed description of how to determine the rank for time-independent cross-correlations.*

9. l.300 "that minimizes the norm" of what? You could state that you favor minimum initial timing error and minimal slopes (drift).(**RB-9**)

   *Good point. We have now clarified this.*

10. The acronym "SI" appears again in l.452, by which time I had forgotten what it stands for. If it is not used frequently, please just spell it out each time. (**RB-10**)

    *We spelled it out.*

With kind regards,

David Naranjo

# Second review round

## Reviewer B

The revised manuscript is sufficiently improved from original manuscript and besides a few minor points (see below), I recommend publication in Seismica.

**Minor comments:**

1. Abstract, l.14:
...to obtain a...
--> for obtaining a

2. l.685 & 691:
I find it a bit weird to have personal communication with an institute (Alfred Wegener Institute). Please refer to the individual(s) you communicated with.

3. caption of Fig 15:
...plus a correction based on the b value to the skew correction...
--Please rephrase to "plus a correction based on the value of b in the skew correction" and refer to the appropriate equation.

4. Acknowledgement:
...extensive (and adequate) answers...
--Please instead consider:
comprehensive answers
constructive answers
valuable answers
vital answers
crucial answers
extensive, excellent answers

Corrections made by the authors.