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Comments Holger Voemel (15 May 2023 by E-mail):

Hi Herman,

 It was good to see you in Jülich and to be able to discuss your paper in person. Below are the details of what we had talked about and some more comments. At this stage, I would not rush to get this manuscript into the QOS special edition. This work is too important, and we need to get it right. I see too many big ticket issues for which we need to get to agreement.

Cheers

Holger

*Hi Holger,*

*Yes, it was really good to have seen you at Jülich and we had a nice dinner and good talks about different on going issues in H2O and O3 balloone soundings. Particular, it was very fruitful to discuss the new paper in conjunction with yours (HV-2020), whereby our agreements but also our dis-agreements on the correction of IB0 or not had given very good discussions during the dinner.*

*Further, many thanks for your thorough review that helps a lot, and it has sharpen our line of arguments and we will correct or add-on based on your critics, comments or suggestions made. Many thanks for that, also on behalf of Roeland and Deniz. Our comments/replies are within your review as listed below. We also included a set of slides with figures to underline our concept and conclusions, particularly on use of IB0 correction, its small impact on Ss, and the impact of the different corrections to derive final PO3.*

*By the end of May 2023 we will have a new version of the paper available for the co-authors to comment on or further to discuss the topics you raised. Our goal is to submit the paper by the end of June. So, we will have still one month to discuss the pending issues.*

*Cheers*

*Herman, Roeland and Deniz*

**Slides to support some key questions raised on the concept and results presented in the JOSIE-New Insights (JNIS)-Paper contra Vömel et al., AMT, 2020 (Here further refer as HV-2020):**

**Do we need to correct for IB0?**

* Slide #2: Impact on slow stoichiometry factor SS
* Slide #4: Impact on JOSIE 2017 results (Difference on Relative scale:

(PO3\_Sonde – PO3\_OPM) / PO3\_OPM

* Slide #5: Impact on JOSIE 2017 results (Difference on mPa scale: PO3\_Sonde – PO3\_OPM)

**What ist the impact when doubling slow stoichiometry factors Ss (doubling correction of slow current , Islow)? Then the factors would map the factors HV-2020 has used to determine Islow?**

Slides #7 - #10 for JOSIE 2017 for SPC/SST1.0, SPC/SST0.1, EN-SCI/SST0.5 & EN-SCI/SST0.1, respectively . Slide#10 is a composition of slides #7 - #10 in one slide.

**What is the impact of the slow time constant on the Ss (=beta) = slow stoichiometry factor, sensitivity of magnitude of 25 min used in this new paper ?**

Slide #3: Table with Ss whereby  the slow time constant, 𝝉slow , used in the paper is 25 min., has been varied here also by +5 and -5 min.

**Holger’s Review (black) with JNIS-author resply (red/italic: Herman, Roeland and Deniz)**

* I finally understood what you mean by convolution/deconvolution and that this is identical compared to what we had done in our 2020 paper for which you and most colleagues from the ASOPOS group are co-authors. In fact, you use the same equations for the corrections of the two time constants. It is important to make this clear to the community. In many places in the manuscript you write something like „new method developed here“. Instead you should always refer to the method developed in Vömel et al. (2020), which is where we developed it first. Furthermore, since it’s the same thing, it would better to call the method the same thing, i.e. time responses correction and not TRRM. Readers will otherwise get the impression that this is a different method from what we developed in 2020, which it isn’t. Likewise, using convolution for calculating the slow reaction component and deconvolution for correcting the time lag of the fast component introduces new terms for what has already been described.

The methodology of this work is common to HV 2020 as far that both are using first order recursive numerical schemes to resolve the slow and fast time responses inherent in the raw ECC-signal. We agree with you, to avoid any misunderstanding in the O3S-community, it is better to stay in line with HV-2020 TRC (Time Responses Corrections) acronym for the new methodology. Therefore we decided to follow the TRC acronym instead of TRRM. However, the numerical schemes used here are based on a more general numerical approach to resolve the first order non-linear differential equation of the ECC-current signal which is based on the “law of exponential growth” and we refer from the beginning in the paper to the concept developed by Miloshevich (2004) to resolve the time lags in the humidity signal of radiosondes. A same approach, also based on Miloshevich (2004), to de-convolve the raw ECC-signal to resolve the fast time response was applied successfully by Imai et al, already in 2013. For the sake of fairness, unfortunately, it is to mention that the ASOPOS 2.0 (incl. Tarasick, 2021, and Vömel, 2020) paid rather very little attention to this paper by Imai et al. from 2013.

In contrast to HV-2020, the here presented methodology, uses the more general mathematical terminology of convolution and de-convolution, to show the reader more clearly the basics of the post flight data processing adapted in how to resolve the slow and fast time responses inherent in the raw ECC-signal. This is what we actually do, instead of a kind of ad-hoc use of the numerical schemes as has been done up to now. Last but not least, our TRC-numerical schemes can use larger time steps to de-convolute the signal for the 20-30 s time response through the inclusion of the response function Xd = Exp (-Δtk/τF ). The last may become important when deconvolving historical O3S records which are archived with only 10 s. or larger time resolution. Although, we have referred a few times to the HV2020 paper we will put a reference earlier in the paper.

* We had talked about the only small difference between the paper with me as lead author versus the paper with you as lead author. You maintain a constant IB0 in the correction in addition to the time dependent slow reaction contribution. I disagree with that approach for several reasons:
  + The background measurements in Vömel and Diaz (2010) show an exponential decay that decreases asymptotically to zero with no indication that there is a leveling off at a constant value (IB0) significantly different from zero. The Uccle measurements you show in your manuscript show the same behavior and do not provide any indication that there is leveling off at a value that is statistically significant different from zero. You argue that the IB0 (10 min after powering on after sitting for a while) and after 2 hours after the 5 uA exposure are the same. However, both still show the same exponential decay. I would argue if you waited longer you would get a different value for IB0.

*From Figure 1 it can be derived that when simply doing a 25 minute decay after t=5min (fast signal has become vanishingly small) then:*

1. *IB60 should be 0.045µA x Exp (-50min/25min) = 0.045 x 0.135 = 0.0061 µA, while we actually measured 0.017 µA*
2. *IB120 should be 0.045µA x Exp (-110min/25min) = 0.045 x 0.013 = 0.00055 µA, while we actually measured 0.009*
3. *This means only a 25 min decay cannot explain our observations*
4. *However, by adopting a remaining IB0 of 0.01 µA, and confirmed by IB120 at t= 120 min then IB60 would approach the theoretical calculated IB60 of about 0.0061 µA , namely 0.017µA- 0.009µA = 0.008 µA.*
5. *Anyway, if not substracting IB0, here on the average IB0=0.01µA, then you are dealing with response time constants of larger than 40 min after t= 60min and even more than 60 minutes after t=120 min decay time.*

*The Uccle experiments were using a special electrometer to measure even the small cell currents of a few nA. More than* ***300*** *IB120 measurements show that the remaining current is not simply the result of a 25 minute decay, but has in addition other origin(s). Here, we further refer to the work of Thornton and Niazy (1982 and 1983) who did excellent theoretical and laboratory work on the physical and/or chemical origin of IB0. At that time before the 1990’s IB0 and IB1 were larger (typical 0.06 µA and 0.09 µA when using purified air), and first in 1993 got down to lower IB0-values of about 0.00-0.03 µA. However the difference of IB1-IB0 have stayed the same (about 0.03-0.04 µA) . In our paper, we have interpretated that this is the actual contribution of the slow chemistry to the IB1, and IB0 has a more physical origin (mass transfer of I2 or electron transfer at the cathode surface). We will improve Figure 1 to show the theoretical 25 minute decay more clearly and mark the remaining IB0 after 120 minutes.*

*Our existence of IB0 contrasts to HV-2020’s arguments of not having an IB0 which is based on an extrapolation after 100 minutes, and only on a few ozonesondes. The 2 hours timing in our paper has been chosen because this is the typical duration of an ozone sounding (ascent and descent together), so even if IB0 still continues to decrease after 120min, this is not of relevance for correcting the currents during the flight.*

*Further, Fig. 4 also shows nicely the agreement of the Islow curve when starting with the groundcheck at t=-4000 s. first 10 min zero air, then 10 min 5 µA ozone and then 10 min zero air again. IB1-IB0 agree very well with Islow derived from the convolved I\_OPM x Ss. Also, when subtracting IB0 this shows most consistency of the Ss values derived from the different response tests RT1, RT2, RT3, and RT4.*

* + With the slow side reactions we have identified a mechanism that explains
    - the behavior seen at every station after the 5 uA exposure:

*We don’t have identified the underlying processes or mechanismns at all, only using the art of describing observations as far as possible.*

* + - the tropical tropopause measurements at very low ozone (in particular CEPEX)

*We don’t agree:*

*Up to now the extreme low O3S-CEPEX data and how to re-process them is not really solved by VD-2010 or neither by HV-2020. Both results look reasonable, although using two different approaches of background corrections. Originally Kley et al., 1996, used a constant background before ozone exposure, which was based on the results obtained from simulation chamber experiments using typical the tropical O3, T, P-profiles as observed during CEPEX. As you certainly remember, the CEPEX data were contaminated by very high background currents which were partly even larger than 0.2µA. In general, however, the extreme CEPEX O3 profiles are rather difficult to analyse in order to get a unique proof of concept of best re-processing the data. One should be aware, that when measuring the extreme low raw ECC currents that the current measurements are within or near the detection limit of the ECC-sonde, which is of the order 0.02-0.03 µA (caused by e.g. limitations of I/V converter of the interface board, non-shielded cabling between ECC and interface board, ECC cell is actually not an ideal current source etc. etc.)*

* + - the agreement in total ozone using the correct pump efficiencies (Johnson et al., 2002; Nakano et al., 2023)

*We don’t agree, maybe the dilemma that showed up after 1995, e.g. JOSIE 1996, and later addressed by Johnson-2002 has been only partly solved. However, using new, correct pump efficiencies and correcting for the slow and fast time responses will remain with normalization factors that are close to one, but not exactly one.*

*Correction of IB0 or not is only of importance as a prominent correction for the tropospheric part of the profile, however, it is only a minor factor in the stratosphere.*

*In our work it is clearly shown that you cannot compensate totally by first the subtraction of the slow current contribution and after that in conjunction the deconvolution of Ifast. Neither it won’t solve the different conversion efficiencies inherent to the SPC and EN-SCI when operated at the same SST, this effect is still “alive” and must be taken into account.*

* + - how to better handle operations experiencing very high background currents.

*Up to now there is no clear evidence what the origin is and how to deal with them. O3S-DQA was a first attempt, but at present we have our doubts if this was the right approach. Maybe Newton et al. (2016) can give some indications, however, all is only based on a quickborne laboratory experiment with several limitations from which no solid conclusions can be drawn how to treat high background currents. At present, we would give the highest priority for further experimental work to answer the question about the origin of the high background currents and how to correct for them. HV 2020, and our work does not give a proper answer to this.* ***We will mention this in the paper more clearly.***

* + - No constant background term IB0 is needed for any of these four aspects. Reintroducing IB0 into the equations reintroduces operator error and station dependent variability.

*We think in terms that first the scientific question must be addressed: do we have a IB0 or not. After that then first the question can be raised how to deal with it, what are the consequences. We think we have experimental evidence of the existence of a constant IB0 which is rather constant during at least the 2-3 hours of the ozonesounding. Furthermore, we think IB0 plays a role in the previous point you raised about how to handle ozonesondes launched with high background current measurements.*

* + Even though you use IB0 in the equation, you still use empirical calibration functions to adjust the remaining difference with the OPM. It’s not surprising that the time response correction doesn’t eliminate all differences, but reintroducing an old parameter and then still using calibration functions seems like a step backwards.

*Please, always the best by keeping to the facts. What is forwards or backwards in progress should not be driven by wishful thinking to get the data fit to the OPM without any transfer or calibration functions. Let the facts speak!!!*

* + If you believe that a constant background needs to be reintroduced, it is essential to justify the mechanism that could cause it and show that it makes a noticeable improvement. I believe our 2020 paper made a significant improvement in our understanding of the ECCs and anything we publish should develop from that. If you think using IB0 is needed in the equations, then it requires a discussion of why, a comparison of how the JOSIE comparisons look like without using IB0 and with IB0, and how the derived stoichiometry factors come differ between the two methods. If you can show that using IB0 makes a statistically significant improvement over not including it and if you can justify the mechanism that causes it, then this would be a very valuable contribution. However, this requires a lot of detailed analysis and solid statistics of small numbers and measurement uncertainties. None of this is currently in the manuscript. IB0 is just assumed to be needed. I didn’t see that need in the data when I did the analysis, which is why I am skeptical that it is meaningful to maintain IB0 in the equations. Since you still use empirical calibration functions, I do not think the data support using IB0.

*HV 2020 as a first paper describing a concept that may be successful, however, it has kept many questions open and unsolved or not be treated properly. The JOSIE 2017 results were presented in HV-2020 in such a way that it looks like that HV 2020 has solved everything. However, in HV-2020 all sonde types and SST combinations were lumped together, compensating known artifacts, so that this merged dataset seem to fit very well with the OPM of JOSIE (WCCOS) by using the Ss factors based on an extremely small numbers of laboratory runs (VD-2010). Our work is based on a solid statistical base of many JOSIE and other laboratory experiments. And, unfortunately, the outcome is not that easy to digest as maybe HV-2020 would suggest. VD (2010) and HV (2020) is not addressing at all the differences in having different sonde type and SST’s. HV 2020 is rather suggestive in that it has solved all. Our goal of this paper is not to get things “fit” to the OPM, but first analyse the results, and then asking what is their impact. Our results give clearly evidence that after resolving the slow and fast time responses the observed differences to the OPM are in-phase with the ozone profile itself and indicate that the remaining differences are more connected to the stoichiometry of the fast chemistry, and NOT to the slow chemistry.*

***See also Slides #4 & #5 to show the impact of correction of IB0 or not incase of tropical profiles (JOSIE 2017)***

*We agree, we have to give some solid arguments of having IB0, but simply to ignore it and claim it does not exist as HV-2020 is doing is certainly not a real “proof”. However, we will more clearly give the evidence and supporting arguments in the paper.*

* Your derivation of the slow stoichiometry factor Ss uses the ratio of small numbers with relatively large uncertainties. That makes the uncertainty of this factor large and probably larger than your statistical standard deviation implies. Your approach assumes knowing the time constant exactly. Since we only roughly assume 25 min, this introduces an additional uncertainty. To be independent of the exact value of the time constants, we used long constant levels in the 2010 Vömel and Diaz paper and called this factor “steady state bias”.

*For the response tests RT1 and maybe also for RT2 the Ss values are indeed based on relatively small cell current values (0.05-0.1 µA) to the IOPM (convolved), while, for RT3 and RT4 the remaining cell currents are substantial larger. Nevertheless, the Ss we are calculating of the RT1, RT2, RT3 or RT4 have all very similar values, particularly when subtracting IB0 from the measured currents!). When not subtracting IB0, the Ss values of the first time response interval (RT1), determined from larger cell current values, are now consistently higher than the Ss values determined from the other RTs. This is really a proof of the robustness of the method, even when relatively small cell currents are involved. No reasons to indicate that our Ss values are systematically too low. However, to eliminate any doubts, we have analysed the JOSIE 2009/2010 and JOSIE 2017 data using Ss values which are a factor 2X larger. The corrections in the troposphere are minor and in the stratosphere only 2-3 % but significant differences at SST0.5 and large differences at SST1.0 still remain. SST0.1 (1%KI or 2%KI) are then under estimating O3 even more. Under the bottom line: NO improvement. The key conclusions we have drawn from our analysis remain the same.*

*Further, we decided to use 25 min for the slow time constant as a good approach. It is know from literature (e.g. Saltzmann and Gilbert, 1959) that the slow chemical phosphate buffer reaction scheme is very similar to the kind of reaction mechanismn of H2O2+KI. From own laboratory experiments we have also observed that the typical reaction time constant of KI + H2O2 is 25 minutes to form I2. We have investigated the impact of varying the slow time constant by +/- 5 min. The impact is minor. WE are preparing a graph to show that. You have to be aware of the fact that the I-slow contribution is of the order of about 5-10% maximum, but mostly less, such that a 5 min stronger or weaker convolution of the signal does not make much difference.*

***See also the Table on Slide #3 where we varied the time constant by +/- 5 min.***

Furthermore, the factor Ss depends critically on IB0. This comes back to my earlier comment. I would argue that for this reason these factors are smaller than what we used in our 2020 paper. I’ll come back to this issue in the Figures showing the comparisons, where I believe that we showed a better agreement in the 2020 paper than what is shown in the current manuscript. You write that your factor Ss is smaller than what we derived in 2010; however, because of your use of IB0 that is to be expected.

*This is not the case: Either substraction of IB0 or not before determining Ss is only a minor increase (10%) in Ss (See also above on determination of Ss and small cell currents).But, the subtraction of IB0 makes the Ss less dependent on the used reponse time intervals (RTs). We have investigated this for-and backwards, but no substantial change of our results showed up, they stay stable.*

***See also Slide #2***

* In section 5, you use the OPM to come up with a pressure dependent calibration of the conversion efficiencies, i.e. the ECC sondes are forced to match the OPM using a calibration function. Can you justify, why these calibration factors should be pressure dependent? The liquid chemistry is probably only weakly pressure dependent, which is why this is probably not a good model. On the other hand, our model for both slow and fast reaction path assumes first order kinetics, which is highly temperature dependent. At the moment we ignore this, but this could be a justification to make the empirical calibration functions solution (cell) temperature dependent. This has the interesting effect that the coefficients change slowly as the cell cools, but then more rapidly as the solutions start boiling, i.e. follow their vapor pressure equation, which could explain the deviation at the very low pressures, when the solutions start boiling.

*Actually all our investigations we did were done with time as independent variable. However, to present the results in a common way, we decided to show our results as function of pressure. As long as we did all the JOSIE experiments on a balloon ascent rate of 5m/sec, typical ascent rate in practice in the global O3S networks. In the same way we have also derived the calibration functions as a function of Log(P). But good point to mention in the paper even stronger possible underlying origin(s) of the calibration functions introduced in the paper.*

*Concerning the cell temperature and its potential impacts on the KI-chemistry, the Nernst equilibrium, time responses, all are good points, and definitely good arguments to go for further research. From early research in 1990’s I know that IB0 is getting larger at larger temperatures above 25C etc.etc., most likely drive by temperature dependent diffusion near the cathode……*

*We will spend a short paragraph to that at the end of the paper.*

* Here, are some other bigger issues in the text I noticed.
  + Line 473: You write that your approach is first order general approach compared to Vömel et al. (2020). That’s not correct since you use the same equations and the same approximation when it comes to the implementation. It’s identical (aside from IB0).

*For the convolution scheme you are right, HV-2020 and TRC are the same, but not for the de-convolution scheme, then HV-2020 is a special case of TRC, and only agree in case the time steps are small compared to the response time. Nevertheless, we will correct the 473-476 sentence concerning this incorrectness.*

* + Line 553: Table 2 does not give 25 and 75 percentiles.

*We decided to use MAD (Median Absolute Deviation) as a good metric for further error propagation. As we are using the median (as commonly done in a Whisker plot) instead of the average, the median absolute deviation is a better metric for its uncertainty than standard deviation.*

* + Table 2: It seems to show a median and a standard deviation. The caption says Median Absolute Deviation (MAD). Why would you want to use that? In the summary you point out that the values for the SPC and EnSci sondes are virtually identical. That needs to be highlighted more strongly, since these results highlight where the difference does not lie.

*Thanks for the comment: We will highlight more strongly that Ss for SPC and ENSCI are virtually the same, and thus systematic differences of 3-5 % usually observed between both sonde types with same SST cannot be explained by the slow chemistry.*

* + Lines 565ff: You write “For SST0.5 and SST1.0 there is a direct, almost linear, relation between the magnitude of SS and the buffer strength.” You only have two data points, so there would always be a linear relationship. The discussion how that relates to the SST0.1 is not valid. The argument should be about the SST0.5, where both KI and buffer are different, so it’s not clear what should be expected. We should have a model for what dependency we should expect, but we don’t.

*You are right, almost linear relation between SST1.0 and SST0.5 should be replaced by almost proportional.*

*When claiming that the slow chemistry is mostly driven by the buffer strength then you would expect that Ss for SST0.1 would be vanishingly small. This means another (unknown) competing reaction mechanism (less dependent on buffer strength) is occurring. A probable mechanism is given, but this is highly speculative, however, it shows again that we don’t really understand everything we are observing. This is what we would like to express in the paper, we don’t like to “fit” our results but to describe what we understand and what we don’t understand. Although, in this paper we don’t claim to understand everything and an in-depth analysis this would be beyond the scope of the paper.*

* + In several places, you write that IB1 and IB0 are essential to apply the time response correction. Instead, you should highlight that the understanding of the time response provides a justification for what values should be expected for IB0 and IB1 and that values larger than that are a strong indication that the operations have a problem (filter, cleanliness, sondes, etc.), i.e., the understanding of the time response provides a justification for our QC indicators in GAW 268.

*Good point, we will link the IB0 and IB1 stronger to the QC indicators of GAW#268*

* + Line 678: What is the justification for using these average time constants for EnSci and SPC? Didn’t you use the actually measured time constants? Come to think of it, it would be very useful to discuss the response time constants (show the means and distribution, is there a dependence on when the sondes were made, etc.)

*Good point: we will better justify the time constants used and show their averages and variabilities and trace back to earlier times (JOSIE from 1996-2017). However, as this is really not the focus of the paper and the manuscript already contains a lot of material, we also do want to overload it even more.*

* + Figure 5: Labels and caption seem to disagree. You could point out that we show basically the same in our 2020 paper.

*Thanks, and the latter we will do.*

* + Figure 6: The time response corrected data show difference increasing with decreasing pressure (which is why you use empirical calibration functions later). In our 2020 paper we show much better agreement for the 2017 data (Figure 6). That is an indication for me that the slow time constant is too small.

*At first sight you maybe conclude that, at least the graphs are suggestive in that way, but in contrast see our earlier explanation that we cannot get better agreement by simply increasing Islow by a factor 2 or more, even then the 2017 results stay different. In HV-2020 you lumped all results into one curve, however, most of the sondes in 2017 were SST0.1 (> 50%), SST1.0 (25%) and SST0.5 (20%), each has different results.*

***See also Slides #7-#10 and #11 (composition of slides #7-#10 into one slide)***

* + In Figure 6, the panels or labels seem to be swapped.

*We will improve that*

* + Figure 7: Same comment. In particular this shows the JOSIE 2017 data, where our 2020 paper shows a better agreement.

*See our comments above*

* + Line 816: That again indicates that the steady state efficiency Ss was not chosen well. However, your Table 3 shows the opposite, i.e. the SST0.1 gives the best agreement with the OPM at least at 1000 and 10 hPa.

What you observing above 100 hPa in the stratosphere the improvements are most driven by the deconvolution and Islow and IB0 are minor drivers.

*We will add some graphs demonstrating the impact of the different new corrections: pump efficiency, Islow, IB0 and de-convolution on the new methodology. Then you can see clearly that the impacts of the buffer and the slow current is less important than we always have assumed. Actually, we could already have known this before, e.g. during the discussions and graphs shown during the O3S-DQA workshops (2011-2012) by better interpretating the absolute differences (mPa) of the sondes compared to the OPM, which are much stronger in phase with the shape of the ozone profile itself than it would be with a 25 minutes convolved cell current signal. Particularly when you would do the de-convolution already before Islow substraction this will show up clearly. Then you see clearly the dominating signature of the fast current and that only a minor contribution of the slow chemistry can contribute to it.*

***See also Slide #5***

*We have shown that the buffer has an impact on the slow current but at same time also on the fast current, and thus on the fast stoichiometry. The Saltzmann & Gilbert (1959) chemical reaction schemes (Appendix A of our paper) maps these findings very well.*

***See also Slide #6***

* + Table 3: This table seems to indicate that the difference between the SPC and EnSci sondes using same solutions is larger at 10 hPa than at 1000 hPa. It would be good to see a plot of that altitude dependence. Does it depend on the ozone concentration? On pressure? On temperature? I didn’t find any discussion of that, but this would be very valuable to understand better.

*Good point: we will add a paragraph to explain that more clearly. The altitude dependence of Table 3 can be seen in the scatter plots, with the linear regression lines illustrating this pressure dependence. Also the plots in the supplementary material showing only the mean relative differences (and not the individual points) for different sonde type/SST combinations in one panel together is already providing the plot you are asking for.*

* + Figure 8: Labels are messed up.

*We will improve that.*

* + Table 4: The uncertainties seem to be extremely optimistic given the noise of the data going in. I assume this is an artifact resulting from using the constant as “1+a”.

*Actually the uncertainties are the standard error given by the linear regression algorithm used. We used three different, independent, algorithms and we came to the same standard errors. These are so low because of the high number of points involved.*

* + Line 904: Figure 9 does not support the statement that agreement lies within +/- 1%.

*We will take care about that*

* + The uncertainty discussion is missing that values are now correlated because of the time response correction and the required smoothing. In addition, the uncertainty of the actual time constants is missing. Their impact is highly gradient dependent. A proper error propagation seems to be missing.

*Good point: we will improve that*

* + Table 5: For the fast and slow cell current you refer to an equation that doesn’t apply here. I wouldn’t know which equations you intended.

*We will correct/improve that*

* + Line 1123: This is not correct. The “steady state bias” was derived from steady state levels, not from upward steps. This difference is not the reason for the different values, but rather the use of IB0.

*The different Ss values we have derived are substantial lower and cannot be explained neither by IB0 subtraction nor by the small residual currents in RT1—RT4 when determining the Ss values.*

* + Line 1147: How can you say that? What evidence do we have about the partitioning between slow and fast cell current that could support that? The use of IB0 could also explain the origin of the calibration functions.

*We will elaborate and explain this more clearly*

* + Line 1153: IB0 is not the major source (it doesn’t exist), but rather knowing the time constants.

*We will explain this more clearly: we are eliminating bias effects but the amount of noise (uncertainty of the IB0 or Islow) is still there and of same order of magnitude as IB in the conventional method. Further, we should be aware that in the UT, particularly in the Tropics we are observing very low currents that are almost close or within the detection limit of the ECC-sonde.* The uncertainty of the slow time constant on the profile uncertainty has been studied by varying it between 20 and 30 minutes.

* + Line 1166: This statement is not correct. The contribution of the slow reaction is essential in the tropical UT and in getting a good agreement near the top of the profile. Correcting the time lag of the fast reaction is important to get ascent and descent into agreement. I wouldn’t say that any is less important, they just play different roles.

*We will explain this more clearly by using new graphs demonstrating the impacts of the different corrections done in the new method.*

* + I noticed many small issues, which I didn't point out. I assume the next iteration will take care of these already.

*Yes, we expect to have a new version ready by end of May 2023, our plan is still to submit by the end of June 2023.*