The paper by Dølven et al. entitled "Response time correction of slow response sensor data by deconvolution of the growth-law equation" deals with the reconstruction of an ambient signal from a slow response/low pass filtered sensor time series. They present the theoretical framework of their approach and apply it to three examples, a step change idealized toy model as well as a laboratory and a field experiment. While the examples center around a slow response (equilibration-based) methane sensor, the scope of the respone time correction method goes beyond those specific applications and can be applied to a wide range of similar scenarios (as the authors note in the introduction).

What makes this work unique is that it (1) derives information on the level of smoothing that should be applied to the reconstruction of the deconvolved signal from the data themselves, and (2) provides an uncertainty bound to the reconstruction, all while having only minimal requirements of number of input parameters/assumptions on sensor behaviour. This sets it apart from previous work (e.g., Miloshevich et al., 2004) with similar goals, and the current work promises to have a large impact on the field going community.

The work is logically structured and written concisely and to the point, sometimes a bit brief considering the likely non-math/signal processing audience of the article. Nonetheless, it is one of the most excellent works I reviewed recently.

Comments

1. In your simulated data and toy model, you use a measurement error proportional to u_m plus a constant noise floor term. After reading the full paper, I see where this comes from (your particular application/example), but do not think that it serves your objectives here as outlined in section 2.1 (l. 134ff.). While the lagged inflation/deflation of the estimate's $\hat{u}_a$ uncertainty is well explained and understandable in the field example (Figure 6d and corresponding text), it raises question marks and creates wrong conclusions here, early on in your derivation, e.g., "*So it seems that the method accumulates/amplifies noise terms with time, as the $\hat{u}_a$ uncertainty envelope grows and grows with time (Figure 3a and c). What would happen if there were a longer 0-period of say time 50 instead of time 5, would the error envelope be huge already at the step change? Can the method only be applied to short pieces of a longer time series??*"

   Instead, I'd suggest to use only a constant measurement error term in the toy model for simplicity, to illustrate that your method does not suffer from accumulation of noise and (in such a case) can keep a nearly constant $\hat{u}_a$ uncertainty. *However*, ...

2. I find it hard to understand and believe that (e.g., with a constant σ_j) the uncertainty of the reconstruction $\hat{u}_a$ does not have/show a dependence on the magnitude of correction (e.g., difference between $\hat{u}_a$ and $\hat{u}_m$). I.e., that the reconstructed signal of a sensor pretty much in equilibrium (in

no-gradient regions) should be as certain or uncertain as the reconstructed signal where the sensor experiences a strong gradient (and needs a strong correction).

Intuitively, with small corrections required (no-gradient case), sensor noise should not be of paramount importance (just slight amplification, reflected in smaller estimate's uncertainty envelope), whereas with large corrections (strong gradient case), the same level of sensor noise should have a stronger impact on the reconstructed signal (more strongly amplified, so larger estimate's uncertainty envelope), shouldn't it?

The figures 2 and 3 tend to support this intuition: "Overshoots" in the reconstructed signal $\hat{u}_a$ tend to be a more prominent issue in strong gradients (here: step change, which violates the smoothness assumption as noted in the Appendix A, agreed. But the same is true if u_a has a "smooth" strong gradient within Δt_j resolution). Similarly, fit residuals (Figure 2d) show a different character at the step change/strong gradient, where they are more coherent (and more coherently wrong) across multiple t_j (suggesting $\hat{u}_a(t_i)$ uncertainty should be larger), whereas they show only high frequency fluctuations around a zero-mean in the later no-/small gradient (suggesting validity of a smaller $\hat{u}_a(t_i)$ uncertainty).

This is probably the most important point that needs to be addressed by the authors (and testifies to the otherwise really excellent work): Should the uncertainty on $\hat{u}_a$ not be larger when there is a large correction compared to a small one?

3. The authors present examples where there are highly-resolved observations together with a very long time constant. This is arguably a very favourable case for response time corrections, where the high resolution allows for a lot of averaging, thus keeping noise amplification low.

   The authors should add a comment in their manuscript on more poorly-resolved scenarios, where τ_{63} is closer to Δt_j and not orders of magnitude between them ($\tau_{63} \gg \Delta t_j$). Is there a limit (e.g., to the applicability, utility, ...) and if so, what to watch out for?

4. As outlined in the introduction, the method may find application in a wide range of fields and settings (e.g. in profiling, on moving platforms, ...). Among them are scenarios, where there is a difference in scale across the time series, both in expected dynamics (e.g., deep ocean with less dynamics vs. surface layer with more dynamics) or in measurement resolution (e.g., a lower and a higher resolution Δt_j part).

   Please add some comment on how to deal with such irregularly spaced scenarios: Would you recommend to split such a time series and use a specific model Δt_i for different parts for better resolution, or rather keep it as one (with uniform scale Δt_i) for better L-curve analysis? Or instead of an evenly sampled grid t_i (l. 71) stitch a few pieces with different Δt_i sample spacing together?

I understand that generic recommendations are hard to do, so if you want, imagine a profiling scenario in the open ocean where response time is on the order of 100 s and measurement resolution varies from 500 s from 2000 m to 1000 m depth (ca. 20 samples), to 100 s from 1000 m to 300 m depth (ca. 80 samples), and finally to 20 s from 300 m depth to the surface (ca. 150 samples).

5. Side question: Can a Δt_i smaller than the max. Δt_j be selected by the regularization? From your example in Figure 3d it seems like it.

6. With your approach in general, is there a difference between full series or subseries application (given same parameters and neglecting edge effects)? I.e., in case of very long time series (and thus a large matrix $\mathbf{G}$), is there a trade-off of splitting the time series to avoid memory problems?

7. Code and data availability: Providing the code of this manuscript will be of tremendous help to anyone trying to apply this excellent work. For code repository, I would however discourage a "static" manuscript supplement, which does not allow for bug fixes or feature updates, and instead use a code repository where this is possible (e.g., github/zenodo or similar).

Minor comments and typos

- l.69: Please add a reference (e.g., to a textbook?)
- l.106: "The constant $\gamma \gg \sigma_j^{-1}$ is a numerically large weighting constant" Like 10x numerically large or like 1000x numerically large? (Does it matter?) Please add some guidance.
- Eq. 8: $k_i + (\Delta t)^{-1}$ when $i = 1$ and $j = i$ Typo?
- Eq. 15: This is not normalized by N (as N changes with different Δt_i), correct?
- l.175: remove "is"
- l.211: Worth adding an equation for this (like Eq. 15)?
- l.242(ff.): Please check the authors guidelines on how to format numbers in exponential notation (I'd have expected $5.747 \cdot 10^{-4}$ s^{-1} or similar).
- l.248: Correct equation referenced?