A review of "Fractional relaxation noises, motions and the fractional energy balance equation" by Shaun Lovejoy, revised version.

General comments

This new version of the manuscript puzzled me and it took a lot of effort to get back to it. Indeed, after having developed several key questions (state of the art, nonlinearity, Gaussianity, simplifying assumptions, spectral techniques, originality), my previous review invited the author "to proceed to a thorough revision that will better build upon the present state of the art that could produced a terser paper with more rigorous, parsimonious mathematics". Unfortunately, the new version of the manuscript is as long as the previous one, and the number of equations only reduced from 127 to 117, which remains at odds with more compact and rigorous papers on similar topics. One reason is that the present manuscript still reproduces mathematical developments that has been developed for the more complex problem of finite initial time and became classical, whereas this paper deals only with the simplified assumption $t_0 = -\infty$. Moreover, this is applied to a very particular case of (ordinary) fractional differential polynomials of "degree" n=2, whereas (partial) fractional differential polynomials of higher degrees have been already formally investigated, including with Fourier transforms (e.g., Podlubny, 1999). Although the author took into account some of these remarks, this duplication is maintained rather than fully acknowledging that the framework of fractional differential equations has been developed for decades across a wide range of disciplines, as evidenced by a number of key reviews.

Contrary to what the author claims, geophysics (especially nonlinear geophysics), cannot be seen as a world separated from physics, mechanics and mathematics and should be well aware of the general state of the art. Furthermore, the questions raised were not about the fact that "more steps and explanations than would be usual in a mathematical – or statistical physics journals", but merely not to forget that these steps were already carried out. Having a goal of application to a particular case in geophysics, does not exempt from any of the aforementioned obligations, including terse mathematical developments.

A second reason is that the use of Fourier techniques remains quite limited despite the author's claim to have taken into account the suggestion on the (potential) bringing-in of Fourier techniques. This mainly concerns the (new) appendices that are rather tentative with many inconsistencies and a cumbersome algebra. Readers still have to reach Sect.3.5 and Eq.65 to get a first information on spectra, whereas it is particularly trivial in the studied case. We are therefore still far from the authors's assertions of "the systematic use of Fourier and Laplace techniques" and "the Weyl fractional derivative […] is *naturally* handled by Fourier techniques" (emphasis added).

There are two new concerns: the reference to Budyko-Sellers type models turns out to be extremely weak and the section 4 does not fully address the prediction problem.

Overall, despite improvements including partial recognition of early results, I remain rather dissatisfied and can only suggest deepening the revision and eliminate inconsistencies.

A new sample of detailed comments

Sect.1 (L31-)

L 90-93: This surprising statement seems rather irrelevant for most nonlinear geophysics, in particular it lacks empirical support.

L153: The link/relationship of the studied model with the conceptual Budyko-Sellers type Energy Balance models seems extremely weak as it completely lacks the space dimension that is central to the interest of these models, in particular the albedo feedback that can trigger nonlinear bifurcations. This calls into question L94-96.

Sect 3.2 (L 476-)
Due to a lack of reorganisation of the paper, Sect 3.2 presents expansions that are primarily addressed in Sect.3.5 (and the corresponding appendix A). Their abrupt introduction in Sect.3.2 is mostly opaque, as is their discussion.

Sect.3.5 (L654-)
I recommended to "start with a developed sub-section 3.5 on Fourier space". This is still not exactly the case, although some steps have been taken in this direction, particularly within the new appendices A and B. However, instead of simplifying the necessary algebraic developments, most of the new ones are devoted to numerous expansions that are scarcely used, as well as a tour of classical special functions. Ironically, the popular (generalized) hypergeometric functions are not directly included, but the G Meijer function is not forgotten. The main problem is that there are inconsistencies, see detailed comments on the new appendices A and B.

L656-657
"…However, it is easier to determine it directly from the fractional relaxation equation…"
is typical of the orientation of the previous versions of this paper, which should not be maintained.

L659
The online equation depends on the precise definition of the Fourier transform, which is not given

L668
Here and at other places, there is a (surprising) confusion between $<|\hat{\gamma}|^2>$ and its spectrum, contrary to Eq.3.

L.674-677
Eq.67 is trivial consequence of Eq. 66, and in fact a demonstration of Parseval's theorem rather than a consequence of it.

L681
A well-oriented paper would rather inverse the implications from Fourier to the physical space

Sect.3.6 (L685-)
This is a lengthy section, with numerous graphs instead of a selection of a few significant ones. This is presumably due to the fact that it is hardly modified from previous versions, therefore with limited inputs from new understandings, while remaining rooted in rather general considerations.

Sect.4 (L796-)
I disregarded Sect.4 in my previous review, simply because of the many requests for clarification of previous sections. Unfortunately, like Sect.3.6, this section has hardly changed. The consequences are worse for this section: having paid some attention to spectra might have reminded the author of some works on predictability, which rather show the present "prediction on past values" is not optimal in the physical sense. This is hidden by a tedious algebra (Eqs.70-73) performed on the ad-hoc predictor $\hat{Y}_\tau$ and its error E_τ with respect to the noise $Y_{H,\tau}$ (surprisingly, the sub-index H is kept only for the last quantity), in particular to highlight an orthogonality between $\hat{Y}_\tau$ and E_τ. This

property is wrongly assumed to ensure $\hat{Y}_\tau$ to be "optimum", whereas it only ensures $\hat{Y}_\tau$ to be an orthogonal projection of $Y_{H,\tau}$ on a given subset and therefore to be optimum only with respect to this subset. At best, this is a necessary condition, but not sufficient. The main question is to define the optimal subset.
Furthermore, a fundamental difficulty is that the chosen predictor is defined with the help of fractional integrals over the past of the noise $\gamma(t)$ ($t\leq 0$), not using observables over the past (e.g., $Y_{H,\tau}(t)$ $t \leq 0$). There is no indication how the noise $\gamma(t)$ ($t\leq 0$) is inferred from given observable(s). The title of the section is therefore inappropriate, as no prediction is actually made. This is in fact confirmed by the applications and figures presented in this section: they do not correspond to prediction from the past, but are limited to comparisons of prediction skill indicators of fGn and fRn (see comments on these indicators below).

L802-803
"The emphasis on past values is particularly appropriate since in the fGn limit, the memory is so large that values of the series in the distant past are important"
L841-844
"in the limit H→1/2, we have perfect skill for fGn forecasts (this would of course require an infinite amount of past data to attain)."
These are two examples of confusing sentences (in various places of the paper) on long range memory. It should be made clear whether or not there is a divergence (at $-\infty$), as the consequences are totally different, as ironically pointed out by my reference to a finite date of the Big Bang (and the Earth). This has unfortunately not been understood. Contrary to the author's assertion, it was worth calling attention to this approximation and so it remains.

L831
Because of the above questions on the "optimum", it is doubtful that $S_{k,t}(t)$ (Eq.76) is an appropriate score indicator. Note that the subindex k is not explained.

Sect.5 (L911-)
Conclusions should be numbered 5, not 4. They should focus on the results of the paper, instead of having lengthy elaborations on the potential applications to geophysics, to which the present paper does not contribute at all. Because of the above remarks, the list of results need to be considerably revised. This may lead to a more appropriate title of the paper.

L 943-945
The mention of stationarity without any indication of its type/order is rather surprising and misleading, especially with respect to climate. Moreover, this there is no direct link to the fixing of the initial time at the Big Bang date or later. For instance, if the former remove transients, it means that transients exist.

L946
"Beyond the *proposal* that the FEBE is a good model for the Earth's temperature…"
As already mentioned, this paper does not contribute to that proposal.

Former appendices A and B
I brought them into question and can only positively appreciate their removal.

New appendix A

It is devoted to evaluate the correlation function with the help of the inverse Fourier transform of the spectrum. Instead of discussing what is really needed for physics and what would be the simplest mathematical techniques to use, this appendix plunges immediately into mathematical details of an inverse Laplace-Fourier transform along a given contour in the complex plane (there is twice a typo “-ve plane”). Moreover, these details are far from being consistent. For instance,

- the contour presented in L1011-1012 is not consistent with a branch cut of the power law z^H
- this branch cut is furthermore confused with given poles (L1016-1017)
- half of poles are forgotten, whereas they are essential to ensure an important symmetry of the solutions
- L1022 the form of Eq.A3 is hardly understandable, especially presented in a vague way as being “a contribution” of the poles
- L1034 Eq.A4, the “convenient coefficient” D_n could have been introduced more conveniently after Eq.A12, presently in L1067
- L1048-1059, Eq.35 is certainly not the simplest way to compute V_H (with the help of a two fold integration), as well as its expansion
- L1074, the identity of Eq.A15 is exact, but what does bring the introduction of the Hurwitz-Lerch transcendental function? By the way, the definition given is only that of the Lerch transcendental function
- L1101-1112 requires to consider the exact definition of the Hurwitz-Lerch transcendental function.

<u>New appendix B</u>

This appendix is devoted to the special case H=1/2 that has been already investigated by Mainardi and Pironi (1996) under the term “ fractional Langevin equation”. The author mentions it briefly “for some early results”. Unfortunately, instead of starting from these results, as well as those in appendix A, one starts more or less from scratch with the help of a tedious algebra of round-trip transforms between physical and Laplace space. Furthermore, the computed correlation does not match early results.