A review of “Fractional relaxation noises, motions and the fractional energy balance equation” by Shaun Lovejoy.

General comments

Despite the reference increase, this revised version has still important gaps on the state-of-art and therefore remains unclear on the truly original contributions of this manuscript. There has been indeed an abundant literature on fractional differential equations, in particular on the relaxation-oscillation equation that is the topic of this paper.

The choice of this linear equation is debatable with respect to the focus on nonlinear geophysics of this special centennial issue. In this respect, there are several rather surprising statements, such as the second part of the sentence: “The choice of a Gaussian white noise forcing was made both for theoretical simplicity but also for physical realism” (L.171-172). This is unfortunately in direct agreement with the oft-quoted, ironic Chester Kisiel’s pray to the theoretical hydrologist: “Oh, Lord, please, keep the world linear and Gaussian!”.

Similarly, the claim of “the paucity of mathematical literature on stochastic fractional equations (see however [*Karczewska and Lizama*, 2009])” (L.78) is in contradiction with various review papers, in particular the Physics Report of Metzler and Klafter (2000) that focuses on “various generalisations to fractional order [that] have been employed, i.e. different fractional operators [that] have been introduced to replace either the time derivative or the occurring spatial derivatives, or both” and has more than 300 references. More generally, the author’s emphasis on opposing fractional vs. integer order differential equations, both linear, seems rather outdated.

The tentative argument in favour of having an “enormous memory” with the help of a lower integration bound $t_0 = -\infty$ of the so-called Weyl fractional integration/differentiation (which in fact could be traced back to Liouville (1832)) is overstated, while it basically corresponds to an over-simplification, not only with respect to the finite date of the Big Bang, but also to Earth climate. Contrary to the author’s claim that “the interval between an initial time = 0 and a later time t […] is the exclusive domain considered in Podlubny’s mathematical monograph on deterministic fractional differential equations [Podlubny, 1999]” (L136-138), this monograph, as several others, does deal with the Weyl fractional integration/differentiation and a more careful reading of it might have helped to simplify and make more rigorous the present manuscript. Let us clarify that a lot of efforts had been spent for the finite t_0 case (e.g., works of Gorenflo, Mainardi and collaborators) due to the fact it was much more difficult than the (negative) infinite case: in fact, it required to define a new fractional derivative (Caputo, 1967) to handle the initial conditions, whereas the classical Riemann-Liouville failed to do it. All these important technicalities vanish for the (negative) infinite case, basically because the Laplace convolution reduces to a Fourier convolution.

A paradox of this paper is that it claims to be innovative by focusing on the Weyl fractional integral/derivative (“the key novelty of this paper is therefore to consider the FEBE as a Weyl fractional Langevin equation”, L.914), while being lost in many mathematical details (e.g., Mittag-Leffler functions) that are rarely necessary for this simplifying case (e.g., the composition of fractional integrals/derivatives is then commutative), as well as not taking advantage of other structural simplifications resulting from the combination of the linear and Gaussian assumptions (e.g., linear stability). On the contrary, the (potential) bringing-in of Fourier techniques is mostly limited to spectra in the short sub-section 3.5 “Spectra”.

It seems that the main results are the scaling behaviours of the studied noise and motion, which the author calls fractional relaxation noise and motion, for small and large time lags. Unfortunately, it is not clear what is analytically obtained and to which level. Often information is missing on important issues, whereas there are numerous mathematical displays. Unfortunately, mathematical rigour is not always there, contrary to mathematical pitfalls that bring into question what is really

obtained (see examples in detailed comments). A clear synthesis, with a comparison with their classical counterparts of fractional Brownian noise and motion, is unfortunately missing.
The aforementioned problems are amplified in the sections on simulations and prediction and make them very difficult to evaluate before these problems will be solved. A final general comment is that the author's claim that the studied noises and motions are generalisation of the Brownian ones is not obvious. Indeed, what could be the new generality gained with their help? On the contrary, an important and generic property has been lost: scaling. This is a direct consequence of the introduction of a characteristic time (presently hidden by the non-dimensionalisation of equations) due to the presence of two time derivatives of different orders (H and 0) instead of a unique one for (fractional) Brownian noises (H or 0). With no surprise, the case of fractional differential polynomials of "degree" n>2 has been already formally investigated (e.g., Podlubny, 1999). This just illustrates that there are many ways to obtain different (approximate) scaling regimes on various frequency ranges. By the way, this also points out the physics of the problem well beyond the mathematical details that submerge the present manuscript. This is also in agreement with the fact that present numerical results on scaling are disappointingly simple compared to the heavy mathematical tools used in this paper.

Overall, the aforementioned problems, as well as the sharp contrast between this long paper (56 pages, 127 equations) with much more compact and rigorous papers (e.g., Karczewska and Lizama (2009)), invite 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. However, not only the mathematics need to be considerably cleaned up, but the main and challenging issue is to define a new "key novelty".

<u>A sample of detailed comments</u>

- introduction: the space-time fractional integration/differentiation for multifractals are surprisingly forgotten as well as others reviewed by Metzler and Klafter (2000), although being more general than the present factional time derivatives;
- Eq. 2 does not provide the Riemann-Liouville fractional derivative (but in fact the Caputo fractional derivative), furthermore no other equation does it;
- L.250: there are many reasons that an integration is not in general the inverse of a derivative, despite this is often considered to be true, including by the author;
- in Eq.3 and equations that follow, the Weyl derivative symbol could be simplified in D_t^H since other fractional derivatives are not used (except in appendix A);
- L261-263: see general comment on this so-called generalisation;
- step-response function of the noise: in the present case (see general comments on the simplifying case $t_0 = -\infty$) it is in fact an impulse-response function of the motion, while in other cases it has much less generality and is much less generic than an impulse-response function. Therefore, it would be simpler to use only impulse-response functions. In particular, Eq.13 is immediate.
- Eqs.15-18 are classical (this should be said without any ambiguity) and are in fact a mathematical detour that is not indispensable, see below, especially comments on Appendix A;
- L.330: it is the Mittag-Leffler functions $E_{\alpha,\beta}$ that are often called "generalized exponentials", not the Green functions;
- L.332: see previous comments on step-response function and there is no difficulty to take care of possible divergences;
- L.341, equation without label: the symbol $G_{\zeta,H}$ is not defined and cannot inferred in a unique way from Eq.16), hence the origin of its r.h.s. is rather mysterious;

- Eq. 17: the leading term is missing, the summation should begin with n=0 , a precise reference should be given to the corresponding theorem that yields only a limited series, not an entire function as displayed. This is particularly important for H=1.
- L.347: poor display of t^{-H}, the present comment is unclear since only this kind of expansion could be expected;
- L367-369: this claims does not seem reasonable because Karczewska and Lizama (2009) worked on the more complex case of a finite t_0 and a complex vector-valued process, hence beyond the framework/approach of the present paper;
- sect.2.3:
  - it should rather begin with Eq. 36-c (with possible divergences) rather than from Eqs.19-20, which are furthermore written in a complicated manner to obtain a simple centralisation of the motion (Eq.21). In the latter equation, there is a one-way implication between the two equalities… which therefore should be stated in the reverse order.
  - Eq.34-b is a direct consequence of Eq.35 and $<dW(s)dW(s')>=ds$
  - there is a change of notations (U-> Y, Q->Z) that is not so helpful (only due to the introduction of the ad-hoc pre-factor N_H in Eqs.19-20);
  - most developments constitute a mathematical detour, furthermore with too many small variations. In particular, the so-called Haar fluctuations of Y are merely fluctuations of their integrals Z. It rather adds a distracting jargon than anything else and should be forgotten.
- section 3:
  - Eqs.40-41:
    - it is rather obvious that the normalisation coefficient K_H is an imaginary number on the range $-1/2 < H < 1/2$ (and a real number on the range $1/2 < H < 3/2$, contrary to the claim of L.490), which is at odd with the non-negativeness of the structure function;
    - one then wonders what was really done in the following, since this expression of K_H could not have been used;
    - as well as what is done outside of the range $-1/2 < H < 1/2$ and for the structure function of the noise (L.468 is rather ambiguous and/or inconsistant);
    - it seems the sign error results from an error on the argument of the sinus, which is indeed different according to another approach;
    - in any case, a lot of information is missing on how Eq.40 is obtained;
  - most developments of the section 3, particularly those around Eqs.40-41, as well as those of of Appendix B, would be greatly simplified if it would start with a developed sub-section 3.5 on Fourier space;
    - Eq.60 does not display relevant functions.
  - Appendix A
    - its goal is questionable since it aims to "use the R-L Green's functions to solve the Weyl fractional derivative equation" (L.1039), i.e., why to use a more complex approach than needed?
    - it seems mostly based on a circular reasoning: ${}_{-\infty}D = \lim_{t_0 \to -\infty} ({}_{t_0}D)$ and R-L is a special case of ${}_{t_0}D$, but forgetting that it is in fact used for a fixed $t_0 = 0$. By the way, the Green function G_0 (Eq. 87 and others) is not defined;
  - Appendix B
    - to go from Eq.90 (improper integral of a series) to Eq.92 (series of improper integrals) requires conditions that are not discussed. They are a priori not satisfied. This explains the divergences of the resulting series;

- obviously, $A_{k,m}$ of Eq.94 does not correspond to $A_{n,m}$ of Eq.93: it rather corresponds to $A_{m,m}\Gamma(m+1)^2$, which is not relevant;
- there is no justification to sum in Eq.94 only the integrals that converge at infinity (L.1069)
- Eq.96 is obviously wrong: its r.h.s. should correspond to the summation of a geometric series (as foreseen from the l.h.s.), which is easy to obtain and quite different;
- it is extremely difficult to accept the statement "Since the series is divergent, the accuracy decreases if we use more than one term in the sum" (L.1080).
- the above inconsistencies bring into question all the claims that follow.