12.12.20

Dear Daniel;

The paper has now benefitted from 5 referees, a personal record. These have been helpful - and with the partial exception of referee 5, they have been uniformly positive. Although detailed comments and responses are given below, stimulated by referee 5, I would like to take this opportunity to make some general remarks about NPG.

Referee 5 is clearly a mathematician unfamiliar with Nonlinear Processes in Geophysics, in particular, the fact that fractional equations have not been much used in geoscience. His comments on fGn, fBm indicate that he is much more familiar with the random walk/ diffusion literature, referring me again to the random walk, diffusion literature that does not contain the precise results needed for the applications to fractional energy balance equation. While this may seem surprising (indeed I personally was surprised), none of the five referees have claimed that any of the key results have been published elsewhere (by this I mean the second order statistical processes of the new processes fRn, fRm). None of the referees have claimed that the results are not unoriginal. Indeed, science constantly throws up new mathematical challenges.

Referee 5 in particular seems uninterested in the geoscience applications and is insensitive to the need to develop the material with more steps and explanations than would be usual in a mathematical – or statistical physics journals. Although perhaps irritating to mathematicians, I think these will prove useful to geophysicists.

The NPG journal was precisely developed so that papers of the present type could find a place.

You as editor repeatedly ask what is the key novelty. It remains the precise statistical properties of the fRn, fRm processes, now made more detailed thanks to the use of Fourier techniques (as suggested by referee 5, but in fact that I had already implemented while waiting for his comments).

-Shaun

Referee 4:

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

The manuscript has been strongly modified. More references have been added and the proposed model is discussed in relation with available literature in the field of fractional stochastic modeling. It is also now more self-contained and is not referring to submitted manuscripts.

I have only one comment: the FEBE is mentioned in the title, in the abstract and the introduction. But after this for about 40 pages the work and presentation is on a new proposal, called fractional relaxation noise (fRn) and fractional relaxation motion (fRm): their definition, their scaling properties, their synthesis and their prediction properties.

It seems that the FEBE (fractional energy balance equation) is only a motivation for this work but is not the main issue. I therefore suggest to change the title to "Fractional relaxation noises and motion: scaling properties and prediction", or something similar. Also I suggest to modify the abstract accordingly. In the introduction it may be indicated more clearly that FEBE is only a motivation to propose such model and that the work is more and the property of such model (which may apply to other situations that FEBE).

Au: I thank the referee for his positive review. Upon reflection, I decided to keep the old title since on the one hand there are now four other papers on the FEBE and on the other to emphasize that this paper is intended more as a contribution to the nonlinear geophysics literature rather than the fractional differential equation literature. Also, the paper was resolutely written for a geophysics audience, not a mathematical one and there are numerous indications, references to the geoscience applications.

Referee 5:

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.

Au: Throughout its brief history, Nonlinear Processes in Geophysics has had a constant theme of using stochastic models for strongly nonlinear systems. In the case of turbulence, the corresponding stochastic models (multifractal cascades) were themselves nonlinear, but often linear stochastic models can be could models for deterministic nonlinear systems. That is one of the arguments for applying the fractional relaxation equation to the Earth's energy balance.

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!".

Au: I have spent several decades focusing on nonlinear stochastic models and have now well documented the fact that macroweather in time (but not space) is the low intermittency, quasi-Gaussian exception to otherwise strongly intermittent, multifractal regimes at higher weather frequencies or lower climate, mega-climate frequencies, see especially [Lovejoy, 2018]. In other words, the model presented in this paper is quite plausibly pertinent for the Earth Energy balance from months to (at least) decades in time scale.

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.

Au: Clearly in the physics and mathematics literature, there has been an explosion in fractional equations in the last decades. This is indeed underlined in the introduction. However, at present, in the geophysics literature (to which this paper is a contribution), there are actually very few papers applying fractional equations. In addition, most of the results that do exist concern random walks and other nonstationary processes. Most importantly, as far as I know, none of the key results needed for applications (the second order statistics) are available elsewhere.

The tentative argument in favour of having an "enormous memory" with the help of a lower integration bound 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.

Au: It is common in both physics and geophysics to ignore the big bang and take time integrals from $-\infty$. I'm not sure that it is worth bringing attention to this approximation.

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.

Au: The aim of this paper is to develop the basic statistical properties of the noise driven fractional relaxation- oscillation equation. In the new version significantly more precise results have been obtained using Fourier and Laplace techniques. As far as I can tell, the results are original and they are needed for applications, now in several papers: [Lovejoy, 2020a; b], [Lovejoy et al., 2020; Procyk et al., 2020].

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".

Au: As indicated above, the aim of the paper was to elucidate the main statistical properties as simply as possible with an aim at the geophysics applications. The more precise results in the new iteration (that are original as far as I can tell) are precisely due to the systematic use of Fourier and Laplace techniques.

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 $t_0 = -\infty$ _-- 1obtained (see examples in detailed comments). A clear synthesis, with a comparison with their classical counterparts of fractional Brownian noise and motion, is unfortunately missing.

Au: It was there, but not clear enough. The new version hopefully makes the similarities and differences more obvious. We clearly establish the difference between fRn and fGn and develop approximations for the differences in their statistics (the new appendix A.3) and in section 4 we systematically compare their predictability skills.

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?

Au: In physical applications of fGn and fBm, the range over which scaling holds is always finite. In applications to the Earth's energy balance this scale has the simple interpretation as a relaxation time. We show much below this relaxation time (which is unity in the nondimensional processes studied here) - that the small scale limit of the new noises and motions studied here – that we do indeed recover fGn and fBm behaviour. The new generality is that it allows treatment of both scales smaller and larger than the relaxation time, including the transitional behaviour.

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.

Au: We have added new emphasis on the physics, especially the fact that the H=1/2 special case can be derived simply as a consequence of the classical continuum mechanics heat equation but with radiative-conductive surface boundary conditions. Thanks to the extensive use of Fourier and Laplace techniques, the analytic and numerical results are significantly stronger in the revised version.

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".

Au: The key novelty is the (now quite full) derivation of the statistical properties (series expansions about the origin, asymptotic expansions) including predictability properties needed for monthly and seasonal forecasting. In the previous version of the paper, only the leading terms in the expansions were given. If these results are available elsewhere, please advise.

A sample of detailed comments

- 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 fractional time derivatives;

Au: Multifractals were discussed in several places, the term appeared 7 times. Metzler and Klafter (2000) discussed random walks and do not give the results discussed here, we have now referenced this paper, thank you.

- Eq. 2 does not provide the Riemann-Liouville fractional derivative (but in fact the Caputo fractional derivative), furthermore no other equation does it;

Au: Thanks, I have modified the definition and improved this paragraph accordingly.

- 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;

Au: This property was not used in the further developments and is now noted as suggested.

- in Eq.3 and equations that follow, the Weyl derivative symbol could be simplified in since other fractional derivatives are not used (except in appendix A);

Au: At the referee's suggestion, we removed the old appendix A. However there are very few uses of the fractional derivative symbol so that leaving the notation is not so unwieldy, it has the advantage of underscoring the differences with respect to the usual applications.

- 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) 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.

Au: Thanks for the comment. I have added the information about $G_{1,H}$ being the impulse response for the motions, although this could be a little misleading since there is the nontrivial issue of low frequency divergences that motivate the present development.

An additional reason for spending time on this mundane issue is that the Energy Balance literature often uses the step response because it give direct information about approach to thermodynamic equilibrium: in energy contexts, it is easier to interpret physically than the impulse response.

- 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;

Au: Yes, from a strictly mathematical point of view it is not needed, but the paper was written for geoscientists for whom this will be new. The figure is also quite useful as is the asymptotic *expansion (eq. 17). Eq. 18 underlines the rather special nature of the usual Energy Balance Equation. I thought it was clear, but I have underlined that this is classical.*

- L.330: it is the Mittag-Leffler functions 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;

Au: Thanks for the correction on the term "generalized exponentials", for the divergences, at the very least, $G_{1,H}$ rather than $G_{0,H}$ is useful for the numerical simulations described later. We have indicated this.

- L.341, equation without label: the symbol is not defined and cannot inferred in a unique way from Eq.16), hence the origin of its r.h.s. is rather mysterious; $G_{\zeta,H}$.

Au: Thanks, there were some words missing as well as the equation number. It should now be fine.

- 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.

Au: For 0<H<1 and 1<H<2 (as indicated), Eq. 17 is correct: the n=0 term for G_{1H} was written independently to emphasize this asymptotic limit. We have nevertheless rewritten it as suggested by the referee. The H = 1 case is given in eq. 18.

- L.347: poor display of t^{-H}, the present comment is unclear since only this kind of expansion could be expected;

Au: Yes, as expected now indicated.

- L367-369: this claims does not seem reasonable because Karczewska and Lizama (2009) worked on the more complex case of a finite and a complex vector-valued process, hence beyond the framework/approach of the present paper;

Au: Thanks, we revised the sentence accordingly.

- 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.

Au: I appreciate that it could be possible to start with the noise (eq. 36) and derive the motion (eq. 19). However, I have followed the usual route following Mandelbrot and Biaginni et al. I think that it makes the divergence issues more transparent. In eq. 21 , I have reversed the order as suggested.

- 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 NH in Eqs.19-20);

Au: The text attempts to consistently derive the noises from the motions (rather than the inverse) so that as indicated, 35 is derived from 34. It is then explicitly stated (line 445) that the derivation could have started the other way around as the referee suggests, although the divergences would need particular care. The normalizations are introduced so that the results may be directly compared with the more familiar fGn properties; the latter are often <u>*defined*</u> *in terms of their (normalized) second order statistics (not as solutions to fractional equations as here).*

- 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.

Au: The aim of the paper is to develop results useful for geoscientists seeking to apply the results to the Earth energy balance. Over the years, the use of Haar fluctuations derived from Haar wavelets, has been very helpful in clarifying climate variability over huge ranges of scales. Indeed, it helped to point out a missing factor of 10^{15} *in the standard model of atmospheric variability [Lovejoy, 2015]. Therefore the referee is therefore correct that from a strict mathematical point of view, the results on Haar fluctuations are not needed however, for NPG readers and energy balance applications, they will be valuable.*

- section 3: - Eqs.40-41: - it is rather obvious that the normalisation coefficient is an imaginary number on the range (and a real number on the range, contrary to the claim of L.490), which is at odds with the non-negativeness of the structure function;

Au: The problem was a sign error in eq. 40, 41, they have been corrected. Clearly we agree: it was indeed correctly stated (line 490) that for $H>1/2$ $K_H^2<0$ *which is the same as saying* K_H *is imaginary.*

- one then wonders what was really done in the following, since this expression of could not have been used;

Au: Due to Gamma function identities, there are numerous ways to write the normalization factor, converting from one form to another led to the sign typo. The correct sign was used throughout.

- as well as what is done outside of the range and for the structure function of the noise (L.468 is rather ambiguous and/or inconsistent);

Au: The normalization is quite standard for fGn, fBm; as indicated often the latter are defined with this normalization. This has now been indicated.

- it seems the sign error results from an error on the argument of the sinus, which is indeed different according to another approach;

Au: Yes, see above.

- 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 Appendix B, would be greatly simplified if it would start with a developed sub-section 3.5 on Fourier space;

Au: The new more complete derivations (starting in Fourier space) in the new appendix A should answer this question.

- Eq.60 does not display relevant functions.

Au: It was for applications, but now eliminated.

- 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: and R-L is a special case of , but forgetting that it is in fact used for a fixed . By the way, the Green function (Eq. 87 and others) is not defined;

Au: OK, I have eliminated the old appendix A.

- 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; $t-H$ $t0$ KH $-1/2 < H < 1/2$ $1/2 < H < 3/2$ KH $-1/2 < H < 1/2$ $-\infty D = \lim\ t0 \rightarrow -\infty$ _($t0D$) $t0D$ $t0 = 0$ $G0$ − 3
- obviously, of Eq.94 does not correspond to of Eq.93: it rather corresponds to , 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. Ak,m An,m $Am,m\Gamma(m + 1)2$ − 4

Au: I have completely replaced the old appendix B with new material based on Fourier and Laplace techniques that enabled me to obtain full series (including asymptotic series) expansions of both R_H, V_H. The new material is now also included in a highly revised section 3.2.

References

Lovejoy, S., A voyage through scales, a missing quadrillion and why the climate is not what you expect, *Climate Dyn.*, *44*, 3187-3210 doi: doi: 10.1007/s00382-014-2324-0, 2015.
Lovejoy, S., The spectra, intermittency and extremes of weather, macroweather and climate, *Nature Scientific Reports*, *8*, 1-13 doi: 10.1038/s41598-018-30829-4, 2018.
Lovejoy, S., The Half-order Energy Balance Equation, Part 1: The homogeneous HEBE and long memories, *Earth Syst .Dyn. Disc.* doi: https://doi.org/10.5194/esd-2020-12, 2020a.
Lovejoy, S., The Half-order Energy Balance Equation, Part 2: The inhomogeneous HEBE and 2D energy balance models, *Earth Sys. Dyn. Disc.* doi: https://doi.org/10.5194/esd-2020-13, 2020b.
Lovejoy, S., Procyk, R., Hébert, R., and del Rio Amador, L., The Fractional Energy Balance Equation, *Quart. J. Roy. Met. Soc.* , *(under revision)*, 2020.
Procyk, R., Lovejoy, S., and Hébert, R., The Fractional Energy Balance Equation for Climate projections through 2100, *Earth Sys. Dyn. Disc.*, *under review* doi: org/10.5194/esd-2020-48 2020.