Biogeosciences Discuss., 9, C1328–C1332, 2012
www.biogeosciences-discuss.net/9/C1328/2012/

Biogeosciences Discussions

Interactive comment on "Inverse method for estimating respiration rates from decay time series" *by* D. C. Forney and D. H. Rothman

J. Lichstein (Referee)

jlichstein@ufl.edu

Received and published: 21 May 2012

General Comments:

Using data from litter decomposition experiments, the authors show that decay models that assume multiple, discrete, decay rates are not statistically robust. A method that estimates a continuous distribution of decay constants is preferred on statistical grounds. Assuming a continuous distribution also seems preferable on biological grounds. The continuous distribution is closely approximated by a lognormal distribution for most datasets.

This is a well-executed and useful study. The biological and statistical arguments in favor of a continuous distribution are compelling. My main concern is that the presentation is too technical/mathematical for many practitioners who would benefit from learning about this work. There are three sections to the Methods: (1) discrete decay rates, (2) generalized continuous distribution, and (3) lognormal approximation. The third is the most important for practitioners. If presented in the right way, the lognormal approximation might be widely used in the future. Method 2 is probably too complicated to ever be widely used. So basically, Methods 1 and 2 are presented primarily as a "set-up" for Method 3. In this sense, the mathematical details of Methods 1 and 2 are irrelevant for most readers. The details of Methods 1 and 2 should be moved to an Appendix for the (very few) readers who wish to study them. The main text should explain Methods 1-2 (and associated Results) in non-technical language. You just need to make the heuristic points: (1) the discrete method is not robust to noise, and yields unrealistically low and high decay rates that are statistically poorly constrained and biologically difficult to interpret; and (2) the generalized continuous method works better. You can do this without ever using technical terms like "inverse Laplace transform" in the main text. Save it for the Appendix. The more you use terms like this, the more readers you will lose before they even get to Method 3. If not you, someone else will probably publish a "user-friendly" version of this paper that will be cited many times. I would prefer to see you do that now, with this paper. I appreciate the mathematical rigor of your work. But with some extra effort, you can make this paper both rigorous (by judicious use of Appendices) and readable for a broad audience.

As for Method 3: Since this is the Method that you advocate for wide use, it seems important to make sure that readers "get it". To this end, you might flesh out the method in more detail, using intuitive language to accompany the equations. Another suggestion would be to provide code to implement the method in R. This is not necessary (and I don't think it should factor into the Editor's decision on the manuscript), but again, someone else will probably do this at some point if you don't.

A second concern that should be addressed in the Discussion: Most carbon-cycle models (e.g., that are coupled to climate models and included in IPCC reports) assume temperature- and moisture-dependent decay rates. How can the methods/results in this paper be used to inform these ecosystem process models? One might assume that there is a distribution of decay rates (determined by litter properties), and that the parameters of this distribution shift depending on the environmental conditions. How can soil C dynamics, as treated in the models you cite on page 3798 (lines 7-8), be modified to incorporate your findings? A complete answer to this question is probably beyond the scope of this paper, but it would still be good to place your work in a broader context by at least attempting a partial answer in Discussion.

Specific Comments:

Page 3797, line 7. "when comparing state-of-the art…": I think you mean that the treatment of NPP in these models is more sophisticated and mechanistic than the treatment of organic matter decomposition. I agree, but the way you word this is confusing. The wording implies you are comparing different models to each other, rather than different processes within a given model.

Too much technical, poorly explained jargon in Intro (e.g., "disordered kinetic model"). Is it really necessary to use this kind jargon? If so, define the terms in more detail. If not necessary, eliminate the jargon.

Page 3802, line 7: Do you mean "Equation (2)"?

Page 3802, lines 14-16: Please clarify exactly what the "m" dimension is. Is it the length of the time series?

How does this paper differ from the Forney and Rothman (2012) citation? What is the new advance in the present work?

P. 3804: Please describe the LIDET data in more detail. Is each dataset a time series for a single type of litter? If so, how would one apply the methods presented here to a real ecosystem, where many types of litter are produced? Would one estimate a distribution of decay rates for each litter type?

Fig. 1A and 3E. Please indicate what exactly "mass remaining" refers to. Mass of what? A single type of litter (presumably, at one LIDET location)? Or is the total mass of a mixture of litter types shown in the figures?

Page 3804, line 29: The text refers to Fig. 1c, which does not exist.

P. 3805, line 11: What are the 3 needed parameters?

P. 3806, lines 5-6: Wouldn't it make more sense to distribute the mass among pools with similar decay rates, rather than distributing the mass among all pools?

Fig. 1B: If I understand correctly, the multi-pool model identifies discrete classes, which should be represented as probability point masses. But the figure appears to show continuous probability densities (albeit restricted to narrow regions of the x-axis). Why? Does Fig. 1B show parameter uncertainty, as is shown (I think) in Figs. 2A-G?

P. 3808, Eqn 9: Since the regularization parameter, omega, is a free parameter, there seems to be a risk of over-fitting the data. In essence, I don't think this is very robust method for separating signal from noise (which is inherently difficult). Nevertheless, I could accept that the method probably works about as well as other 'good' methods.

P. 3812, Eqn 12: Firstly, couldn't you simplify this expression by replacing the integral with g(t), as defined in Eqn 11? Secondly, the least-squares minimization problem assumes no temporal autocorrelation in the errors. This can bias the estimates of parameter uncertainty in the presence of autocorrelated errors. It seems desirable to allow for autocorrelation in the estimation procedure, e.g., by assuming the time series is a sample from a multivariate normal distribution.

P. 3813, line 1, and elsewhere in the paper: I am confused how/why you identify certain datasets, but not others, as decaying exponentially. To my understanding, all of the datasets are assumed to decay exponentially, but not necessarily with a single decay rate. By "decay exponentially," do you mean a single rate?

Interactive comment on Biogeosciences Discuss., 9, 3795, 2012.