Nonlin. Processes Geophys. Discuss.,
doi:10.5194/npg-2016-60-RC3, 2017

Nonlinear Processes in Geophysics
Discussions
Open Access

Interactive comment on "An upper limit for slow earthquakes zone: self-oscillatory behavior through the Hopf bifurcation mechanism from a model of spring-block under lubricated surfaces" *by* Valentina Castellanos-Rodríguez et al.

Anonymous Referee #2

Received and published: 28 March 2017

This paper addresses the problem in geophysics of finding the depth of the border between a thin seismogenic layer and an underlying stable layer. This is important because the border area is thought to be where slow earthquakes occur. Determination of the region size must be based on a knowledge of the mechanism for such events. Hence a correct estimation of the border depth holds out the possibility of predicting such earthquakes.

I take as my starting point the idea that the geophysics community welcomes work of an accomplished theoretical standard, that good theory is to be valued and nurtured (that is not clear in other areas, such as parts of engineering, where design considerations can drown out detailed theory).

So I can only be disappointed in this paper. It labours a standard analysis, misses out both key points and important results and then leaves the reader bereft of a valid geophysical conclusion. Also the exposition is poor (concepts not explained well) and the English is, in places, dire.

Where to begin? Well, the 3 page introduction nods in the direction of many different papers both in geophysics and in theory. I am not an expert in the former, but the ground appears well covered, giving a sense of why the area is important and why simple Coulomb friction will not do, for sound geophysical reasons. But the theory references are less convincing. For example, I would never refer to Avrutin et al for the Hopf bifurcation.

Printer-friendly version

Discussion paper

Section 2 introduces equations (5) which are the main subject of the paper. Again, the justification for the system seems fine. But then the problems begin. It took me a while to work out that section 2.2 is about the unforced (not *unperturbed*) equations. But it is the treatment of the characteristic polynomial (not *polynomial characteristic*) that grates. In situations like this, you have to immediately state and use the Routh Hurwitz (RH) criteria. These are the industry standard used to determine the necessary and sufficient conditions needed to ensure that the equilibrium of the unforced equations is stable. You do not get that from this paper (true, RH is mentioned later on p11, but only in passing).

Section 3 begins badly. I accept that earthquakes are nonlinear and that this nonlinearity comes about because of friction. But the eigenvalues correspond to the linear problem and only tell us about the equilibrium solution. We need to work a lot harder to understand the role of nonlinearity.

What should really happen is that the RH criteria should be front and centre of the paper. These give you the clear limits on parameters that guarantee a stable equilibrium. You have 3 eigenvalues, so for stability you need the real part of all 3 to be negative. If that happens, your unforced quake dies away exponentially - no oscillations. Then if you are happy with damped linear oscillations (are these meant to be the slow earthquakes?), you need two of the eigenvalues to be complex conjugate (they have to be conjugate as your characteristic polynomial has real coefficients). But again the real parts of all 3 eigenvalues have to be negative. Then as you vary parameters, you want to avoid the real parts of the complex conjugate pair crossing the imaginary axis (otherwise you get a Hopf bifurcation).

All of this is in the paper, but so hard to find and interpret. What is needed is a clearer structure, starting with RH and then some good bifurcation diagrams.

Now you have your Hopf bifurcation and you get oscillations in your unforced system. But now the question is: is it a subcritical or supercritical Hopf bifurcation? The latter is not very interesting. The former is very dangerous, where a small perturbation before the Hopf is reached can lead to either decaying oscillations or a jump to a large sustained oscillations in the system. Which do you have in this paper? It seems to me to be of extreme importance to know which it is, since the monitoring of a crucial parameter by geophysicists would be doomed to failure if it were a subcritial Hopf (a sustained quake would be triggered way before you reach what you think is your danger point).

Section 3.3 reintroduces the forcing term and consists of some numerical simulations. But here a big opportunity is missed. There is a wealth of theoretical work done on forced systems near Hopf bifurcations (going back many years), whose results show clearly that everything depends on what type of Hopf bifurcation you have in the first place. I found this section to be very poor. One paper with some good references on this topic is Yanyan Zhang and Martin Golubitsky *Periodically Forced Hopf Bifurcation* SIAM J. Applied Dynamical Systems 10 (4) 1272–1306 (2011).

The last section is a discussion of the results, devoid of any real connection with geophysics. Perhaps that is too much to ask. But I feel it is not too much to ask that straightforward theory be applied correctly.

Finally, the writing is poor. Ideas from dynamical systems theory are mangled and confused in a way that I would need an hour to unpick. Same with the English: too many examples to deal with. Let me just mention the second sentence of the abstract: “The mathematical springblock model is generated by considering the Dieterich-Ruinas‘s friction law and the Stribeck‘s effect." would work much better if it were something like “The mathematical spring-block model includes Dieterich-Ruina‘s friction law and Stribeck‘s effect" or “The mathematical spring-block model includes the Dieterich-Ruina friction law and the Stribeck effect." As it stands, the sentence incorrectly conflates two nouns, uses an awkward construction, uses the wrong possessive and misspells a name (Ruina, not Ruinas).