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Nonlinear Processes in Geophysics Discussions

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.

Valentina Castellanos-Rodríguez et al.

valentina@cimat.mx

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We appreciate the comments and suggestions from the Anonymous Referee number 2. We thank for his comments on the article. These suggestions and recommendations are very timely and very supportive to enrich this document. We take his observations into account and they were attended as follows:

Referee's comment. 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.

Author's replay. We take the suggestion into account.

Author's changes in manuscript:
The reference from Avrutin et al. (2014) was removed. Section 3.3 and the references section include the following quotation:
Yanyan Zhang and Martin Golubitsky Periodically Forced Hopf Bifurcation SIAM J. Applied Dynamical Systems, 10 (4), 1272 - 1306 (2011).

Referee's comment. 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).

Author's replay. The reason we do not address much about the sufficient condition for stability is because we are more interested in the necessary condition (13), which is fulfilled for values close to the Hopf bifurcation in the unstable regime. Specifically we are interested in oscillatory behavior, so we analyze the form of eigenvalues to focus our study on complex conjugate eigenvalues with positive real part.
We agree to restructure section 3.1. The Routh Hurwitz criteria must go in this section.
Author's changes in manuscript:
In Section 2.2, line 11, page 6, we added: with tau (t) = 0. This indicates that the analysis is about unforced equations.

Lines 7 and 8 on page 8 are relocated on line 14 of page 7; Followed by lines 1-8 on page 11 and fig 5. Lines 20-22 of page 7 are inserted and rephrased into one of the conditions for the Routh Hurwitz criteria.

Referee's comment. 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.

Author's replay. We agree. We need to work a lot harder to understand the role of nonlinearity in many of the geophysical phenomena.
In the spring-block model, the logarithmic term in the Dieterich-Ruina friction law has introduced greater difficulty in solving the problem. Due to the nonlinear term, analytic integration has not been possible, and even numerical solutions present challenges because of the logarithmic term (Erickson *et al*., 2008). The linearized system analysis is very useful to describe some features of the nonlinear system about steady state solution (Gu *et al*., 1984; Shkoller and Minster, 1997; Erickson *et al*., 2008).

It is important to say that we started our nonlinear analysis as in Gu *et al*. (1984), Shkoller and Minster (1997), and Erickson *et al*. (2008) determining the equilibrium point. This equilibrium point is of great interest since this is where the steady sliding occurs. This solution is very important in the analysis of the dynamic system that characterizes the earthquakes mechanism. The linearized system allows us to simplify our analysis. The linearized system gives a qualitatively correct image of the phase portrait near $x^{\star}$, since it is a sink or saddle point in Perko's definition (2001), so for the original nonlinear system, $x^{\star}$ it is really a sink or a saddle Point (Strogatz, 1994, Page 151, see the references therein).

We are interested in the saddle point which is a hyperbolic point in the sense that the Jacobian matrix has at least one eigenvalue with negative real part and at least one eigenvalue with positive real part. The steady-state solution is a hyperbolic point for parameter values in the oscillatory interval (OR) (Eq. (20) page 10).

Author's changes in manuscript:
We added a clarifying text in section 2.2 after line 15 on page 6: the first paragraph of this replay.
The reference Shkoller and Minster (1997) was added in the references section.
In the introduction of section 3 line 11, page 7 we added:
We use the full nonlinear term in the numerics in sections 3.2 and 3.3.

Referee's comment. 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.

Author's replay. Our analysis does not focus on the stable regime, but on the unstable (Page 7, lines 25-28, page 8, lines 7-11, and Figure 3; page 10, lines 20-24; page 12, lines 1-3). The earthquakes can only be generated in the unstable regime. Our interest is in the unstable regime near the critical nucleation value (near the hopf bifurcation, where parameters approximate values for the critical value of nucleation). Specifically, we analyze the oscillatory region: one of the eigenvalues is negative real and two are complex conjugate with positive real part.

The mechanism of slow earthquakes is not very clear yet, but one of the theories is that they are generated in the transition zone, between the frictionally unstable and stable region, with parameters values near the critical value of nucleation, hence the importance of starting from the hopf bifurcation and analyzing the oscillatory behavior in their environment, particularly in the unstable regime. In previous studies a complex behavior has been observed in this neighborhood. The oscillatory behavior is very sensitive to variations in the values of the parameters; on the other hand Scholz (1998) deduces that there are self-oscillations in that region, therefore we start from the hypothesis that in the region of slow earthquakes there are damped oscillations depending on the values of the parameters, from this statement we derive the proposal of the upper limit of the slow earthquakes zone (page 2, lines 1-4, page 3, lines 6-25, Page 2, Figure 1).

Referee's comment. 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).

Author's replay. From the Hopf bifurcation we varied the bifurcation parameter γ in such a way that the parameter values be either before and after crossing the Hopf bifurcation plane. In terms of the flow in phase space, a supercritical Hopf bifurcation occurs when a stable spiral changes into a unstable spiral surrounded by nearly eliptical limit

cycle. From the Hopf bifurcations shown in the Figure 4 (page 9) all simulations display behavior as in supercritical bifurcation. For example, in the figure attached as a supplement, starting at the Hopf bifurcation $\Pi = (0.25, 0.8, 0.8)$, Figures (a) and (e) show the parameter values have no crossed the Hopf bifurcation. Small disturbances decay after ringing for a while and stable spiral is observed. The block and the driver plate are moving at constant rate $v = 1$, and the relative position is η. This occurs when $\gamma > \gamma_{HB}$. On the other hand for $\gamma < \gamma_{HB}$, Figures (b) and (f) show the parameter values have crossed the Hopf bifurcation. The equilibrium state lose stability and unstable spiral is observed. This type of bifurcation is expected for smooth, non-catastrophic changes. The slow earthquakes are almost imperceptible because the displacement rate is very low compared to ordinary earthquakes and they are generated for parameter values around the critical value of nucleation. This argument and the numerical simulations leads us to infer that the Hopf bifurcation is supercritical within the proposed limits for unforced system. To find chaotic behavior or strange attractors with the non-forced system it is necessary to vary epsilon very far (Erickson, *et al.*, 2008) from the value of the Hopf bifurcation that we are analyzing.
However, Kostić *et al.* (2013)(Srdan Kostić, Igor Franović, and Kristina Todorović: Friction memory effect in complex dynamics of earthquake model, Nonlinear Dyn. (2013) 73:1933–1943 DOI 10.1007/s11071-013-0914-8) found chaotic behavior for small values of Π by introducing delay time in the friction term. They found the two types of Hopf bifurcation depending on the variation of the delay time. Similarly, by introducing the external force $\tau(t)$ a subcritical Hopf Bifurcation could be given for some critical $\tau(t)$ and slight variation of the ε and ξ parameters. Disturbances do not allow the system to remain at an equilibrium point resulting in continuous oscillations or chaos. In the figure of the supplement (c) and (g) $\gamma > \gamma_{HB}$ and the values of v and u remain close to 1 and η on average, whereas in the figures (d) and (h) the range for v and u is wider and variable for the case where Π crosses the Hopf bifurcation. Continuous oscillations are found in both displacement and velocity only by varying the bifurcation parameter. For the analysis of the bifurcation type, for the system (5), the main challenge is the

numerical stiffness, due to the nonlinear logarithmic term. Determining critical value of Π and $\tau(t)$ requires a more concrete study. We limit ourselves to the cases presented in the article, leaving it as opportunity area to explore the system.

Author's changes in manuscript:
The comments about this answer and the graph mentioned will be introduced in the new section 3.3.3 Numerical features from the Hopf bifurcation analysis.

Referee's comment. 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).

Author's replay. We added a brief introduction in section 3.3, page 12.

Author's changes in manuscript. Introduction in section 3.3:
This section aims to numerically describe the oscillatory behavior within and outside the range proposed for the SSO region (Eq. (21), page 10), under forcing and non-forced conditions. We want to prove numerically that the proposed upper limit determines the changes in oscillatory behavior, below and above this. For more theoretical background into the theory of periodically forced systems near a point of Hopf bifurcation, see Zhang And Golubitsky (2011) and references therein.

Referee's comment. 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.

Author's replay. On page 2, lines 5-10, we mention, in a summary and general way, the information that is available from observational and experimental studies for the characterization of SSEs, also the parameters involved in these investigations are mentioned (Watkins *et al*., 2015 ; Marone *et al*., 2015; Scuderi *et al*., 2016).

We agree that the real example would make the paper more appealing to a wider audience, however the parameters considered for slow earthquakes are still being studied experimentally and by means of simulations, but there is still not something precise, and even in simulations it is difficult to work with realistic values due to the logarithmic term, which makes the system sensitive to values of v close to zero, so giving a real example is complicated. The conclusions are given in terms of the implications of the parameters involved, according to previous results. However, an approximation of some of the parameters considered in this article are discussed in Watkins *et al*. (2015). Their study seek to model observations with a simulator to reproduced reported characteristics of SSEs in Cascadia through variations in (A-B), normal stress, and convergence rate. The estimates of these parameters do not adjust to the SSEs in Cascadia with respect to the recurrence intervals. Fluids and variations in the width transition zone might affect the recurrence times, among other factors.

Author's changes in manuscript:
We think that the following should be mentioned at the end of the conclusions (pages 17, 18):

Although this investigation is more related to the proposal of a formal pattern in the study of SSEs, and with a first approximation of the upper limit of the transition zone, this is considered as a preliminary study in order to be applied to the real seismogenic regions. However, the parameters considered for slow earthquakes are still being studied through observations, experiments, and by means of simulations, but there is still not something precise.

The study of SSEs in Cascadia (Watkins, *et al.* 2015) indicates a possible link between the observational and experimental data with the parameters involved in the most of models of earthquake's physic coupled to the Dieterich-Ruina's friction law. The slip amount of SSEs are of order of cm but the average slip amount of smaller events are unknown. The effective normal stress in the range of 3-9 MPa produce fault slip consistent with some observed SSEs, $B - A$ is in the range (0.0015 to 0.003) of the slow slip section. At the top of the slow slip section $B - A$ is 0.003 and 0.001 at the base, $A \approx 0.02$, L is in the range 1-50 μm (real L is unknown), the rate of convergence (10 a 50 mm/year) represents the range of convergence rates of subduction zones where SSEs are observed with GPS. These parameters could vary depending on the region that SEEs occur. On the other hand, the critical value $K_c = (A - B)\sigma/L$ depend on L; viscosity=0.1 (nondimensional) has been used in earthquake models (Carlson *et al.*,1994), but the estimation of the real viscosity depends on the region.

The proposed upper limit for the SSEs zone includes the fluids and oscillation frequency (and consequently, L), through ψ. They might be introduced into the simulations and experiments in order to see which are the implications over the recurrence times, duration and velocity of SSEs in real seismogenic regions. A final step would be using scaling laws for SSEs to determine the real values of parameters included either experimental and/or simulation data, such as the stiffness K_c and viscosity, take into account the specific characteristics of the fault.

Added the reference Carlson *et al.*,1994.

Referee's comment. 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).

Author’s replay. We use the basic theory of dynamic systems, necessary to show the point that concerns us: to propose an upper limit (through the necessary condition for stability), which marks the difference in oscillatory behavior, when this condition is satisfied, and when it fails. This study is a first proposal of a limit for the area of slow earthquakes, obtained from an earthquakes model. From here, we can go deeper theoretically, and apply it in simulations with real data, as far as posible. We are aware that the analysis can go deeper into mathematical theory, but we are also aware that the journal is mainly directed to geophysician and seismologist, so we try to use a language more in line with other authors who have analyzed dynamical systems of the mechanism of earthquakes with mass-spring systems. We hope that with the observations you have indicated, this article had improved on the structure and clarification of what is being investigated.

Author’s changes in manuscript:

Typos, grammar and spelling mistakes have been revised and corrected:

All “et al" corrected to “*et al*".

Page 1, line 2:“Ruinas’s" corrected to “Ruina’s". Page 1, line 2: the sentence “The mathematical springblock model is generated by considering the Dieterich-Ruinas’s friction law and the Stribeck’s effect" was changed and corrected to “The mathematical spring-block model includes Dieterich-Ruina’s friction law and Stribeck’s effect".
Page 4, line 2: “relatives" corrected to “relative", line 11: “velocity function" corrected to “velocity dependent", line 17: “increase" corrected to “increasing", “decrease" corrected to “decreasing"
Page 5, Line 18: “taking account" corrected to “taking into account", lines 19-20: “third order system differential equations" corrected to “first order differential equation system".
Page 6, line 11: “has the components" corrected to “is given by"; Page 6, line 23, and page 7, line 15, Page 9, Line 10, Page 11: Table 1, caption: “polynomial characteristic" corrected to “characteristic polynomial".
Page 6, line 22: element 12 of Jacobian matrix is “1" corrected to “0",

Page 7, line 15: “Descarte’s" corrected to Descartes’. Page 13, line 8, Page 16, line 13: “longitude" corrected to “length". Page 7, line 24: “conjugate complex eigenvalues" corrected to “complex conjugate eigenvalue"; Page 7, line 22: “is given a sufficient condition to stability" corrected to “a sufficient condition for stability is given".
Page 10, line 5: “has two conjugate complex eigenvalues" corrected to “has two complex conjugate eigenvalues", Page 11, line 1: “asymptotical" corrected to “asymptotic"; page 11 line 5: capital letter “J" was changed in “jacobian".
Page 15, line 7: “necessary condition for stability. The" corrected to “necessary condition for stability, the" Page 17, line 26: “stablished" corrected to “established". Figure 1, caption: “doted" corrected to “dashed". Figure 2, caption: “de" corrected to “the".

Other corrections:
Page 4, line 3: The variable x assigned for the displacement of the block was replaced by the variable y.

Page 5, lines 3-8: The paragraph was clarified, rephrased, and relocated after

Equation (4); added the term "β_1" to equation (2), it could be 0 if it is considered in Dieterich-Ruina's friction law.

Page 5, line 21: "$F_0(v)$" was removed; page 5, line 27: added and modified "$\alpha = \{\alpha_1, \alpha_2, \alpha_3\}$; $\alpha_{1,2} = \frac{L\beta_2}{v_0^2 M}$, $\alpha_3 = \frac{L\beta_3}{v_0 M}$. The external force is $\hat{\tau}(\hat{t}) = \hat{c}\sin(\hat{w}\hat{t})$, where $\hat{c} = \frac{L}{v_0^2}$ and $\hat{w} = \frac{Lw}{v_0}$". Page 5, line 28: added the term "α_1" to equation (6).
Page 6, line 3: We added the text "The function $f(x)$ on the right-hand side of Eq. (5) defines a mapping $f : \mathbb{R}^3 \to \mathbb{R}^3$. This mapping defines a vector field on $\mathbb{R}^3$. Thus, the system given by Eq. (5) induces in phase space $\mathbb{R}^3$ the flow $(\varphi^t), t \in \mathbb{R}$ such that each forward trajectory of the initial point $x_0 = x(t = 0)$ is the set $\{x(t) = \varphi^t(x_0) : t \geq 0\}$".
Page 6, line 12: added the term α_1 in η; page 6, line 8: added $\hat{\tau}(\hat{t}) := \tau(t)$, $\hat{\omega} := \omega$, $\hat{c} := c$
page 6, line 18: "mean that every solution of the system (θ, u, v)" corrected to "*i. e.*, every solution of the system $\varphi^t(x_0) = (\theta(t), u(t), v(t))$",
Page 6, lines 20-21: "where $f(x)$ is the vectorial field or right side" corrected to "where $f(x) = (f_1, f_2, f_3)$ is the vector field given by right-hand side", line 21: "with $\tau(t) = 0$; and" corrected to "with $\tau(t) = 0$; $(x_1, x_2, x_3) = (\theta, u, v)$; and".
Page 7, line 2: Equation (11) was modified to $\frac{Mv_0}{L} + \frac{A}{v_0} + \beta_2 \mu e^{-\mu v_0} < \beta_3$; Page 7, line 14: "$Re(\lambda_i) \geq 0$ for one or more eigenvalues of $D_f(x^\star)$" corrected to "at least one eigenvalues of $D_f(x^\star)$ is positive, *i.e.* $Re(\lambda_i) \geq 0$".
Page 17, line 20: (Ruina, 1983) was changed by (Daub and Carlson, 2008), last one was introduced in the references section.

Please also note the supplement to this comment:
http://www.nonlin-processes-geophys-discuss.net/npg-2016-60/npg-2016-60-AC3-supplement.pdf

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