



SDI Review Form 1.6

Journal Name:	Physical Science International Journal
Manuscript Number:	Ms_PSIJ_42783
Title of the Manuscript:	An elastic-plastic finite element analysis of two interfering hemispheres sliding in frictionless contact
Type of the Article	Original Research Article

General guideline for Peer Review process:

This journal's peer review policy states that **NO** manuscript should be rejected only on the basis of '**lack of Novelty**', provided the manuscript is scientifically robust and technically sound. To know the complete guideline for Peer Review process, reviewers are requested to visit this link:

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PART 1: Review Comments

	Reviewer's comment	Author's comment (if agreed with reviewer, correct the manuscript and highlight that part in the manuscript. It is mandatory that authors should write his/her feedback here)
<p>Compulsory REVISION comments</p>	<p>This work is an interesting work, however there are some issues to be clearly explained as following</p> <ol style="list-style-type: none"> page4 line9, tridimensional should be three-dimensional; page 4 line 17 defined in [32], should be "defined in reference [32]". page 5, R1=R2=1m, then equivalent radius R=0.5m as deduced from equation(3), and then equation (8) can be deduced as $U_c = \frac{\pi(CS_y)^5}{480E^4}$, is it? so why not deduce it to a simple formula in page 5, the equivalent modulus of elasticity $E' = 0.5 * \left(\frac{E1}{1-\nu^2}\right) = \frac{0.5*200}{1-0.32*0.32} = 111\text{GPa}$(bulk elastic modulus), in page 7 E is E1 or E' in equation(4), it should be pointed out, or else it confuses the readers. page 5, equation (2) is expressed as : $\frac{\sigma_e}{p_0} = \frac{1}{2} \sqrt{[(1-2\nu-2\xi^2(1+2\nu)+2(\xi+\xi^3)(1+\nu)\text{arccot}(\xi)]^2 / (1+\xi^2)^2} = \frac{1}{2} \frac{(1-2\nu-2\xi^2(1+2\nu)+2(\xi+\xi^3)(1+\nu)\text{arccot}(\xi))}{(1+\xi^2)}$, is it? you should make sure this equation. page 5 equation (3) R1 and R2 should be pointed by schematic figure. equation (4) is the same situation. page 5, equation (5) and (6) are got from others or by empirical formula based on experimental data. page 17, figure 3 b),the maximum Mises stress reaches 1.04E9Pa which is higher than yielding strength of the steel (915MPa), and Page 18 Figure 4b has the similar result of 378MPa > 310MPa of Al. Under such condition, the increment theory should be employed to explained this phenomenon, but in the article, I didn't find increment theory, can you explain why. In addition, in the model , you didn't introduce the stress-strain relationship during plasticity processing. page 23 Figure 7 shows us the plasticity deformation, however, in the previous page the plastic model and plasticity parameter doesn't be presented. page 29, the author should explain why the curve Fy/Pc is not a symmetrical figure, and what reason causes this displacement (in Figure 12 and Figure 13). Energy loss in equation (9) is only a two-dimensional model, and the energy loss in y direction can't be neglected in the model. page 44-50, in Figure 24-28, actually, the data used in SAW model were also obtained from Abaqus calculating, which has senseless meaning for comparison, isn't it. I suggest to delete these comparison. 	<ol style="list-style-type: none"> Corrected Corrected The viewer is correct in the math, but the reduction to what he/she offers would limit his/her Eq. specifically to this work, while Eq. (8) valid for any general case of R1 and R2, including the current work. The value of UC is given in Table 2 anyway, so change is made here. The information appears on page 6, however, subscripts are now added to parameters in Table 1. The equation in the paper guarantees a positive value for radical and the entire calculation, while the reviewer's Eq. does not. No change is made. See clarification highlight on page. 5 "For two bodies 1 and 2 that may have distinct radii and material properties,..." No. They were obtained from the same reference [38], which is added. The reviewer must have missed the statement on page 6: "...in order to help convergence, a bi-linear material model with a 2% strain hardening based on the elastic modulus is used." That explains the point made by the reviewer. A slight change of wording appears now on page 6. In Fig. 7 the residual plastic strains are shown (not "plastic deformation"). In general, I do not understand this comment. It is clearly explained as stated in the text – see the entire paragraph: "The normalized vertical reaction force, Fy/Pc, as shown in Figures 12 and 13, show a nearly symmetric pattern about the x/R axis (vertical alignment). As interference increases the maximum forces occur earlier in the sliding progression. This can be attributed to the fact that plasticity is initiated earlier as interference increases...." Before Eq. 9 it is stated that it is "the net energy loss in sliding." It is the work done by Fx. No changes are needed. No, SAM did not use ABAQUS at all – SAM's results have been obtained from a dedicated self-coded program (see Ref [33]). The comparisons are of value. No changes are made.
<p>Minor REVISION comments</p>		
<p>Optional/General comments</p>		