Letter to the Editor: On "Contact mechanics of real vs. randomly rough surfaces: A Green's function molecular dynamics study" by C. Campaña and M. H. Müser, EPL, 77 (2007) 38005[1]

The paper by Campaña and Müser compare, of the many theories for randomly rough surfaces, only Persson's and Bush et al, BGT. Like other recent papers, they often assume the original Greenwood and Williamson (GW) theory [1] to be inaccurate, but *unfortunately do not* test it, assuming BGT to be its better version. The original GW however is, I will show below, still the best method today, with less assumptions than any other model, including general constitutive equation. The paper contains questionable statements, including a dubious interpretation of results:-

1)   On the value of the coefficient of proportionality area-load κ, the results show similar and *not* reasonable degree of error for the two theories (20-25% for the ideal surfaces), and much better agreement for the real surface with BGT than Persson's, a large 50% in defect. There seems to be some reluctance to put the results of the real surface in Fig.3. In independent numerical results [2-5], Persson's coefficient has been found always in defect, and sometimes by large amounts, whereas BGT seems to have generally closer correspondence. Notice our contribution [3] in particular is a very accurate full halfplane elasticity solution in 2D which permits high mesh refinement, not involving asperity approximations.

2)   the authors embarrass me by considering only my "re-vitalized" version [2], and not the original GW theory [1]. Also, I am not sure what they mean by saying *the most advanced (and potentially accurate) GW model based calculations* which however has "*uncontrollable approximations*" – could they clarify that please with some scientific evidence? My contribution is very close to the original extremely simple spirit of GW, works for any distribution of asperity heights: it is simply a GW in discrete form. On the contrary, the BGT is the most advanced form of GW, with additional assumptions to use Gaussian random process theory. The calculation of the original and the re-vitalized GW are so elementary that they could have easily used it for comparison, and get a direct quantitative impression of the "*uncontrollable approximations*". It must be said that also other authors [4,5] forget about GW theory, and concentrate with BGT, perhaps because BGT's resulting formulae has the same form and is easy to compare to Persson's. However, it is not the only form as suggested on dimensional analysis grounds by [5]: GW in fact predicts an alternative factor to slope, which is the (square root of) ratio of mean summit radii to rms roughness. Naturally, there is some loose relationship between the two dimensionless factors.

3)   Persson's theory is indeed very elegant and elaborate, but why should it be "*less phenomenological than that of Bush*"? Also, it may be prone to future extensions but it is *not* entirely comparable to GW since it only provides partial results and not the dependence of all quantities on separation. So far, it contains stronger assumptions than GW as all numerical results confirm since partial contact is predicted only by some simple features of the full contact regime [6]. The differential equation derived by Persson may in principle hold rigorously also under partial contact conditions, but some terms should depend on the solution itself, so a full solution would probably be like *killing a mosquito with a bazooka*.

4)   The "re-vitalized GW" seems to have been largely misunderstood but is a radically *simpler* approach than all other numerical methods. In fact, at low loads it is a really elementary sum which could be easily applied even for millions of asperities in a matter of fractions of seconds. We are available to provide software code for anyone willing to test it. An example of the results is
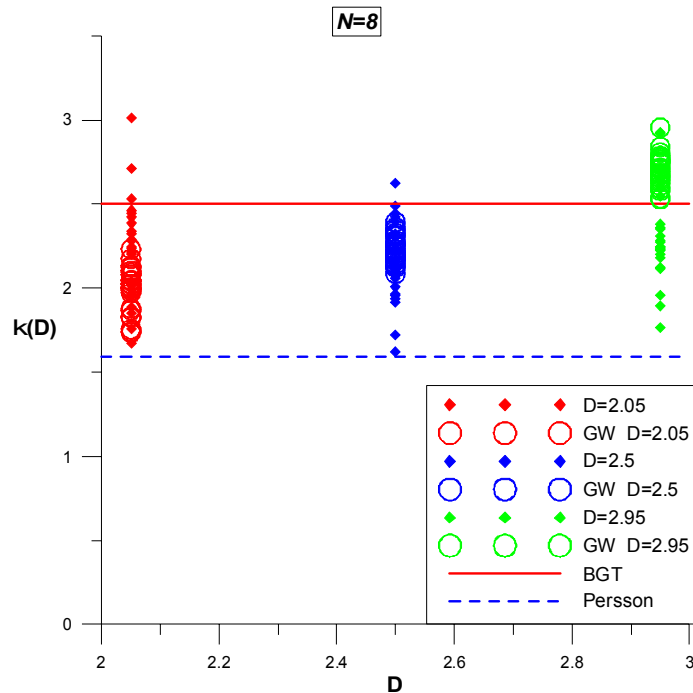
provided by our Fig.4 of [2][2], which shows that with only few asperities in contact (a case which may also be important in practice), both Persson and BGT are largely in defect.

5)   I do not fully understand the authors' definition of the pressure distributions, but they admit they use a method which is not able to find good accuracy at small pressures, as it is evident since their distribution do not start from the origin. My re-vitalized GW model is a convenient alternative method, and found at low pressures big differences with respect to Persson's theory. Incidentally, in [2] a closed form result is obtained to compare the GW prediction for the pressure distribution using different distribution of asperity heights (eqt.17 in the original). For the Gaussian distribution, the resulting limiting form is $exp(-p^2)$ as is Persson's, whereas for the exponential, it would be trivial to find $p \cdot exp(-p^2)$, which is not too different, and still not the exponential form suggested by some authors ---are there simply numerical errors? But do we care?

I confess I also did the mistake of not comparing with the original GW theory in my paper [2]: I remedy here with the present Fig.1 where by fixing the number of terms of the Weierstrass-Mandebrot surface to $N=8$, we show only the results with the larger number of asperities, and hence with smaller scatter. The results of the "re-vitalized GW" are those with rhombs, and once again, it is found Persson's theory is more like a *lower-bound than an actual good estimate*. Circles indicate results with the original GW elementary formula with the appropriate mean radius. It is seen that assumptions in GW work surprisingly well and better than either BGT and Persson, and in general agreement with the results of Robbins' group [4,5] for self-affine surfaces, despite Weierstrass-Mandelbrot is believed to be non-perfectly isotropic, nor self-affine, nor Gaussian.

The idea that what tribologists long called "real contact area" was in fact a scale-dependent quantity for "fractal surfaces" mixed up people's ideas so much as to consider that GW theory could not deal with this, preparing the way for the hope to new theories, to which Persson's theory elegantly and timely responded. It is unfortunate that this circumstance had not been noted in the other excellent contribution, that by Jack Archard [7,8]. Persson's theory has temporarily has made people forget about the magnificent original GW 40 years old paper, but I think time will give justice to this.

[2] There is misprint in Fig. 4 since on the x-axis there should be a parameter rms Δ suggested by [4], and not fractal dimension D. This is clear since D cannot be ranging from 0 to 3!

Fig. 1 --- Re-elaboration of results from [2], with additional estimation using original GW (circle)

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