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COMMENT

A Comment on "Meeting the Contact-Mechanics Challenge" by Muser et al. [1]

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The recent paper "Meeting the Contact-Mechanics Challenge" [1] is a very useful and tremendous effort to elucidate and compare various theories and numerical models for the contact of rough surfaces. It is easy to understand why this problem has attracted a lot of interest, as many imagine this as a very basic problem of tribology. The main motivation in the past for these kinds of models of rough contact was a qualitative understanding of linearity in Amonton's law, which, however, is explained easily with a number of models, from the fully plastic one of Bowden and Tabor, to the elastic ones of Archard, Greenwood-Williamson and Persson. It is possible that tribology remains too complex for quantitative modeling, and these efforts focusing on roughness remain largely academic, and so far haven't made much progress in quantitative predictions of wear, adhesion, and friction since the times of Leonardo in 1500 so there is no alternative to use empirical measurements for friction coefficient, wear coefficient, or such like (I may have indulged myself in this academic exercise in the past!). In any case, theoretical models have obviously some appeal, and with no doubt, the models proposed by Persson, originally only giving the contact area as a function of pressure in rough contacts of infinite bodies with Gaussian random roughness [2], have permitted more accurate solutions of the *mathematical problem* with respect to the old "asperity" models, or "Archard" or "Winkler" ones, as the Contact Challenge paper clearly shows. Persson's theories build on ideas of renormalization group theory and explore the problem from different perspective (in particular, the idea of "distributions" of pressure, of separations, etc). Persson's theories remain always approximate, and in particular, some aspects of the theories require a number of "corrective or fudge factors," as Wang and Müser [3] themselves discuss at some length in the same issue of this journal, which are inevitably "tuned" on existing numerical solutions, and to specific cases.

However, for a theory to be really useful to the community, it has to be clearly reproducible and as such, its result completely accessible. Since the Contact Challenge paper contains some limited amount of adhesion, it is perhaps more appropriate to only a subset of Persson's theories, like ref. 33, 34 of [1], but how do we know in advance when we should look instead for Persson [4], Persson and Tosatti [5], or Persson and Scaraggi [6]? From the original papers, the various theories do seem to differ largely. And which one was used here? Why? Has Persson some criteria which we don't know to choose between one and the other? The use of corrective factors also reduces the appeal of a theory, especially if they are not well justified, and there is quite significant use of these fudge factors, whereas there is less attempt in other theories—all other theories in the Contact Challenge could be improved if one starts to devise fudge factors, so in these respects, the conclusions are less strong than they seem.

With reference to the load-separation $p(u)$ curve, despite being an averaged quantity, its accurate prediction is proving quite challenging, and indeed there are various forms of "corrective factors" even for the adhesionless case. Persson [7] (eqt. 20) finds a simple asymptotic form at low pressures p and for the common fractal dimension $D_f \simeq 2.2$ (for purely self-affine spectra)

$$\frac{p(u)}{E^*} \simeq \frac{3}{8} q_0 h_{\mathrm{rms}} \exp\left(\frac{-u}{\gamma h_{\mathrm{rms}}}\right) \tag{1}$$

where E^* is the elastic modulus in plane strain, q_0 is the lower wavevector cutoff, h_{rms} is the rms roughness and γ is a corrective factor which is said to account for a "redistribution" effect of the elastic strain energy in the contact areas. (The elastic strain energy can be written exactly only in full contact, as obviously the entire problem.) Here, the dependence on the large wavevector cutoff q_1 (and hence on magnification $\zeta = q_1/q_0$) seems lost, and only q_0 is relevant.

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Persson obtains relation (1) exactly for pure power law spectrum and there is no mentioning of "roll-off" in the power spectrum, which later on people are using to make the surfaces more "Gaussian," including the Contact Challenge paper [1]: Whether this is realistic or not is a matter of debate: Persson's theory is a "thermodynamic limit," but a tribologist may not know what this implies, and [7] is never quite clear about this approximation. In my humble opinion, when one has a specific surface, it is interesting to know the "thermodynamic limit" response of this surface, but it is equally very important to know the actual response to this surface, and how far it is from the "thermodynamic limit." A normal reader gets the impression when reading Persson [7] that equation should indeed hold for pure power law spectra without roll-off. Indeed, we have obtained some significant discrepancies recently in Papangelo et al. [8] in how there are large deviations due to finite-size effects, and how the multiplier can differ very considerably. We then found Yang and Persson [9] who introduce a more complex approximation for the energy repartition factor, which becomes a function. Perhaps a problem in Yang and Persson [9] is that integral (16) seems to me, when using the most recent version of the corrective fudge factors, to sum to a different value from what is stated in that paper, $\varepsilon = 4.047$.

In an elegant study, Carbone et al. [10] specializing Persson's theory for 1D anisotropic roughness show that the load-separation curve seems almost perfectly matched without the need of any corrective factor in the theory, although the error in the contact area is actually large and increases with respect to the 2D case! This clearly calls for the need for independent corrective factors for the energy (which disappears in 1D, curiously enough, making it weaker the argument about why this correction should occur) and the stress broadening, but the number of corrective factors would increase, and the number of factors on which it could possibly depend also. One example is again in Wang and Müser's [3] findings, about the slab geometry, and finding that rather minor modifications of the functional form of the corrective factors dramatically affect the contact area (whereas they don't show the load-separation results), as shown in their Fig. 7.

But another worry is that our own attempts to reproduce the results of "Persson" theory with the different corrective factors show additional discrepancies: We tried to explore the simplest version, without adhesion, including now Yang and Persson [9]. Sticking to the data of the Contact-Mechanics Challenge [1] we are commenting, we fully take into account of the roll-off in the power spectrum, and therefore cannot use eqt. 1. However, it is still trivial to use eqt. 8 of Persson [7] (the full form of the theory, which we counter-checked with eqt. 1 for the low-pressure end), or eqt. 17–19 of Yang and Persson [9] (we restrain from implementing the full form as it becomes already more demanding to implement, for my own perspective). We find the results of Fig. 1, which also reproduce the reference numerical solution GFMD, and Persson's own submission to the Contact Challenge (kindly offered to us by Dr. Muser), which we call "Persson-Contact Challenge" as we don't know to which of the various Persson's theories we can attribute it. Clearly, the version of Persson's theory suggested in Persson [7] underestimates the pressure by large factors (and this cannot be due to adhesion which would rather explain the opposite). On the contrary, the version of Persson's theory suggested in Yang and Persson [9] now overestimates the pressure by large factors (adhesion was rather weak to explain this); the overall difference between the two versions is about 500% in terms of pressure!

In summary, we would welcome more explanation on:

- how Persson's results were obtained, in terms of actual equations; which version of Persson's theory was used for those parts where adhesion was neglected, and which for those in which it was included. The Muser et al. [1] paper refers to a couple of Persson's theories, ref. 33, 34 in [1], but yet other theories exist and indeed at least two other theories in the case with adhesion [4–6]: Could we see also the prediction of these other theories, or at least know why they were not used?
- if Persson would get from eqt. 8 of Persson [7], or eqt. 17–19 of Yang and Persson [9] the same results of Fig. 1, just as an indication of whether we are missing something obvious, despite we implemented very simple equations (the simple ones as possible are the more interesting than the full versions with double integrals or so), and made effort to check equations carefully. In particular, if Yang and Persson [9] integral (16) should really be $\varepsilon \simeq 4.047$ as stated in Yang and Persson [9] or perhaps more like $\varepsilon \simeq 2.8$.

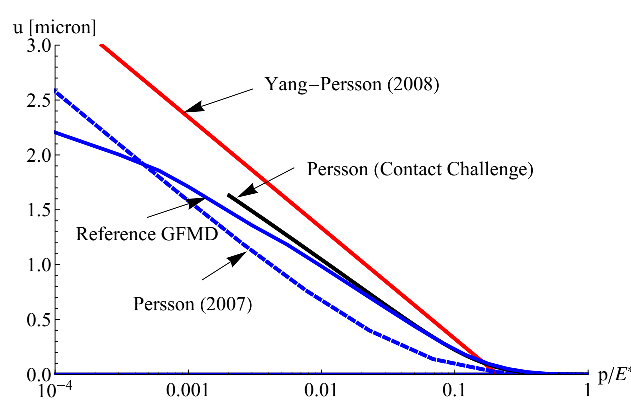

Fig. 1 It corresponds closely to Fig. 12 of [1]: mean separation [micron] against normalized pressure p/E^* We have used the factor $\gamma = 0.48$ and eqt. 8 of Persson [7], or eqt. 17–19 of Yang and Persson [9]

- How general are these corrective factors, in view of Carbone et al. [10] which finds extremely different results for 1D anisotropic roughness, or by Wang and Muser [3] on elastic slabs?

A warning is that since these corrective factors are based on an approximate area of contact, when one use improved estimates for the area of contact, like Wang and Muser [3] try to do, then one should use still the "wrong" contact area. All this makes the situation very confused, and I look forward for some clarification.

We certainly congratulate the authors of the Contact Challenge paper for their effort, hoping our comment is a useful contribution to this debate which should certainly continue with even more "challenging" problems, since after all the very ideal purely *mathematical problem* of isotropic, self-affine, Gaussian, fractal surfaces in nominally flat infinite bodies is only a beginning, although proving complicated enough, so that we could really define it a "contact sport" between academics!

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