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A simplified version of Persson's multiscale theory for rubber friction due to viscoelastic losses

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Abstract

We show the full multiscale Persson's theory for rubber friction due to viscoelastic losses can be approximated extremely closely to simpler models, like that suggested by Persson in 1998 and similarly by Popov in his 2010 book (but notice that we do *not make any use* of the so-called "Method of Dimensionality Reduction"), so it is essentially a single scale model at the so called large wavevector cutoff. The dependence on the entire spectrum of roughness is therefore only confusing, at least for range of fractal dimensions of interest $D \simeq 2.2$, and we confirm this with actual exact calculations and reference to recent Lorenz *et al* data. Moreover, we discuss the critical assumption of the choice of the "free parameter" best fit truncation cutoff.

Key words:
Roughness, Contact mechanics, rubber friction, Persson's theories, adhesion

1. Introduction

After the introduction of the concepts of multiscale roughness (Ciavarella *et al*, [1], Persson, [2]), we have recognized that the real area of contact is very loosely defined and it depends, together with some other physical quantities, on the small wavelength truncation of roughness, which is hard to define with some physical argument, rather than just as a best fit fitting "free parameter".

This led to a proliferation of papers about multiscale roughness (see e.g. Persson *et al.*, [3]) and the debate between the classical asperity models (Greenwood & Williamson, [4]) vs the more accurate Persson model [2], see eg. Putignano *et al*, [5]. Since Persson's theory and GW predict the same dependence of the contact area vs load at asymptotic large separation even quantitatively, it took a long time to realize that the difference exists and is rather in the intermediate range of separations, where Persson's theory certainly is more accurate. Hence, Persson's theory improved one aspect: the area of contact is actually in principle much less dependent on roughness details than what GW theories, and developments, had predicted.

However, it remains now a bigger problem: no reliable estimates can be made of quantities like the real contact area, or other quantities which strongly depend on the cutoff wavelength, like friction in viscoelastic bodies even in the most advanced models which rely on these quantities (although hidden in the much complex formulation) which are difficult to define. Actually, the latest development of Persson's theories which involves friction due to viscoelastic losses as well as a number of other mechanisms (Lorenz *et al*, [6]), suggest the truncation of the spectrum of the surface should be such that the rms slope is fixed to

$$h'_{rms}(q_1) = 1.3 \tag{1}$$

where we have made it evident that the rms slope strongly depends on upper truncating cutoff wavevector q_1. A quite interesting simple result, although there seems to be no clear understanding for such a universally good choice. Other authors also recently (Carbone & Putignano [7]) do not consider the problem solved and prefer to consider the truncation cutoff it as a "free parameter", which may be related "to the micrometer size of the small dirt particles covering the contacting surfaces or, alternatively, can be related to the size of rubber wear particles". It is surprising that at least since Persson [8], the various attempts to model friction in rubber material with quite different choices and models (the number of papers is very large), they all seem to conclude a good agreement with experiments. We tend to think that the reality is in the middle: there is a good qualitative agreement in most of the models, but when one tries to be quantitative, the number of effects is so large that only a certain choice of the fitting parameters in the models, makes the answer reasonable, within the obviously limited and well specified range of experiments to be modelled.

Starting from the two really fundamental contributions of Williams Landel & Ferry [9] who gave a single empirical function known as the WLF

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transform to relate temperature and rate dependence of viscoelastic properties, and Grosch [10] experimental result showing a single "master curve" can describe the temperature and velocity dependence of friction, many authors have attempted to make more "quantitative" models. In fact, Grosch showed that friction on a rough track Fig.1 (solid line) shows two maxima, one related to molecular adhesion with the track, the primary source of friction on a smooth surface (Fig.1 dashed line), at low speeds (and this contribution disappears with adding fine powder like Magnesia to the track), and another due to viscoelastic losses in the rubber at much higher speeds. Grosch [11] has a good review of this, of other experimental evidence (like dependence on normal load which we will briefly mention) and developments for modern polymerization methods and filler concepts with mainly lab work which aims to change the viscoelastic properties to aim at an optimal result of high friction high wear resistance and low rolling resistance.

Heinrich [12] was perhaps the first to attempt introducing fractal surfaces concept in the topic, while at about the same time Persson [8] in a simple paper, suggested the "rubber completely follows the short-wavelength surface roughness profile", introducing some energy balance adhesion concepts which he later developed and yet today are no longer present in his models (because adhesion is considered to be destroyed by large roughness?). In any case, the interesting aspect is that dissipation occurred in a wavelength of order the diameter of the asperity contact area, not defined a priori *but clearly fixed as a single scale in the model*. This led him to suggest that the friction coefficient was

$$\mu = C\frac{\operatorname{Im} E\left(\omega_0\right)}{\left|E\left(\omega_0\right)\right|} \tag{2}$$

where $E\left(\omega_0\right)$ is the complex viscoelastic modulus of the rubber, and $\omega_0 \sim v/l$ is the frequency of the cyclic deformation at velocity v, in the asperity of diameter l. At that time, he concluded that C could be found from GW theory but for "very rough surfaces typically involved in rubber friction, C is of order unity" – a conclusion that we shall reobtain after a long discussion. Also, $\frac{\operatorname{Im} E(\omega_0)}{|E(\omega_0)|}$ is also of the order unity at the frequency where this ratio assumes a maximum, concluding that friction would be of the order unity. Persson [8] was entirely happy at that time of reasoning in terms of a single scale model of roughness, and very surprisingly a posteriori, was aimed at explaining the result for the very smooth track (due to molecular adhesion) with the hysteresis loss, as he discusses the case for roughness of just $100 Angstrom$ and

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similar wavelength (notice therefore the slope is about 1), like in the glass polished with alumina powder of Grosch experiments (see Fig.1).

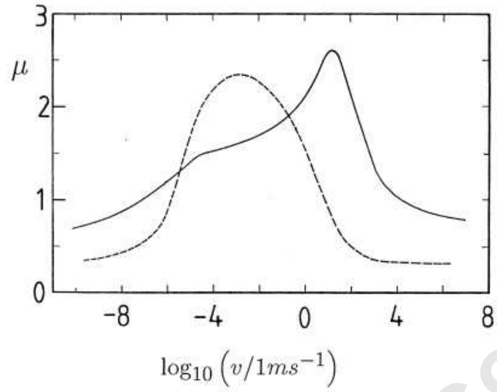

Fig.1. Adapted from Persson [8]'s Fig.3 in turn using Grosch [10] results on silicon carbide paper (solid curve) and a smooth glass surface (dashed curve)

Persson [8] did not comment on silicon carbide paper results of Grosch which, with much higher roughness, with particle size of order 0.01 cm, showed similar value of friction but clearly could not be explained by the simple adhesion/losses model – on the contrary, it is this case where its hysteresis loss theory now attempts to explain (Lorenz *et al* [6]), while adhesion contribution is attributed to shear strength in the contact area which involve newer models. The calculation based on adhesion and full contact showed a dependence on normal load of the friction coefficient (which may be qualitatively justified for smooth surfaces), and a quadratic dependence on the slope (ratio of amplitude to wavelength) of the sinusoid, which however was assumed of the order unity. This calculation was found, with appropriate estimates of the rubber properties, to give correct order of magnitude results as the observed frictional stress in most cases. Today, Lorenz *et al* [6] say that there is "no way" that a hysteretic friction mechanism can explain the low speed friction result (like essentially those in Grosch with glass surface which Persson [8] considered in very good agreement with the theory).

Despite the quite radical changes of interpretation in the models, it remains interesting that assumption of slope of the order of 1 makes most models, still reasonable.

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Popov, in his book ([13] eqt.16.12)[1], arrives at a similar equation as Persson [8] (2), as in fact he uses simple argument based on a single "scale" of asperities (same diameter), but this time he includes the rms slope of the surface, because we know (mainly thanks to Persson [2] theory and the corrective factors that have been suggested (see Putignano *et al* [5]) the dependence of the pressure in the asperity contact on this geometrical parameter

$$\mu = h'_{rms}(q_1) \frac{\operatorname{Im} E(\omega_0)}{|E(\omega_0)|} \tag{3}$$

This obviously holds when we are in the regime of small fraction of contact area, as it is realistic for macroscopic applications. It implies of course that $\mu \leq h'_{rms}(q_1)$ and in particular, the curve follows the dependence on the frequency (and therefore on velocity of sliding) observed by Grosch, and in the middle frequency domain, many types of rubbers have a peak $\frac{\operatorname{Im} E(\omega_0)}{|E(\omega_0)|}$ close to 1.

But naturally one aspect is very often not much discussed: what is really the rms slope of surfaces? This quantity varies wildly at small scales and in principle would grow up to infinity, and at atomic scale it is certainly ill-defined, but from extrapolating of the fractal scalings, it can easily reach a value of 10. In his most recent take on the problem, Lorenz *et al* [6] suggest to take $h'_{rms}(q_1) = 1.3$ and the real reason for this choice is obscure (other than the authors attempt of best fits): clearly, if we took a much higher value, it would be difficult to convince people about its meaning, as already 1.3 makes most newcomers quite surprised. After all, we are discussing of models purely developed at small slopes, which neglects all finite deformation, rotations, plastic or even viscoplastic behaviour. Notice that even with this postulate $h'_{rms}(q_1) = 1.3$, the pressure in the contact areas is of the order of

$$p_{rough} = E^* h'_{rms}(q_1)/2 \tag{4}$$

which means deformations of 65% and pressures near the elastic modulus: this would be prohibitively large for a metal, although for a rubber it may still be a range where, approximately, linear elasticity is satisfied (?).

But the main effort in the present paper is to see if full Persson's multiscale theory is really so different from the earlier simpler models.

[1] This should not be confused with the so called Method of Dimensionality Reduction (Popov and Hess, 2015).

2. A simplification of multiscale Persson theory

Persson has given his main theory for contact mechanics and rubber friction in [2] where friction is due to the hysteretic losses assuming no adhesion at the interface. With some improvements, it proves quite good compared to full numerical simulations involving only this mechanism of friction obviously (see e.g. Scaraggi & Persson [14]). However, it looks quite involved and doesn't show any immediate dependence with respect to the much simpler (2, 3). The friction coefficient depends on the "magnification" or on the truncation wavevector, so we will use the notation $\mu(q_1)$ which makes this clear, rather than μ used by Persson which seems to suggest a true quantity. It reads

$$\mu(q_1) \simeq \frac{1}{2}\int_{q_0}^{q_1} dq q^3 C(q) S(q) P(q) \times \int_0^{2\pi} d\phi \cos(\phi) \operatorname{Im} \frac{E(qv\cos(\phi))}{(1-\nu^2)\sigma_0} \quad (5)$$

where σ_0 is the nominal contact stress, $C(q)$ the surface roughness power spectrum (defined as a function of wavevector q), and the function $P(q) = A(\zeta)/A_0$ is the relative contact area when the interface is observed at the magnification $\zeta = q/q_0$, where q_0 is the smallest (relevant) roughness wavevector, and A_0 is the nominal contact area. Finally, the factor $S(q)$ is a correction factor which at large magnifications, can be taken as $S(q) \simeq 1/2$, and otherwise results from some fitting calculations of the stiffness of the contact (which were done for elastic contact), but is equal to 1 for full contact. The theory also gives

$$P(q) = A(\zeta)/A_0 = \operatorname{erf}\left(\frac{1}{2\sqrt{G}}\right) \quad (6)$$

$$G = \frac{1}{8}\int_{q_0}^{q} dq q^3 C(q) \times \int_0^{2\pi} d\phi \left|\frac{E(qv\cos(\phi))}{(1-\nu^2)\sigma_0}\right|^2 \quad (7)$$

where the argument of the complex module is the projection of the wavevector on the direction of sliding.

Let us consider the outer integral in (5). For small q_0, the contact area starts from full contact, because G is small and one seems to need the full formulation. However, if we assume full contact persists up to a certain q_f, we obtain at that scale that the friction coefficient should be

$$\mu(q_f) = \frac{1}{2}\int_{q_0}^{q_f} dq q^3 C(q) \times \int_0^{2\pi} d\phi \cos(\phi) \operatorname{Im} \frac{E(qv\cos(\phi))}{(1-\nu^2)\sigma_0} \quad (8)$$

where we recognize the square of the rms slope. At intermediate q_f the rms slope remains quite small and a fortiori its square value, and moreover the loss modulus is also quite small, so this contribution (which incidentally would give a simple inverse dependence on normal load) can be neglected if the surface has a spectrum which spans many decades. One could therefore use this model for very smooth surfaces, although not much effort has been produced to test it. In fact, despite the dependence on the normal load is well known, it is not quantitatively or qualitatively in agreement with the previous equation but rather shows a much weaker power law (see Grosch, [11], Fig.1 and Table 1), probably because for very smooth surfaces, the correct model is adhesion-based and not hysteresis losses anyway.

In any case, for these many reasons it has little sense to add this contribution to the integral (even assuming it were correct), because for large spectrum surfaces, the main contribution to the friction coefficient will come from the large wavevectors. The contact area decay with wavevector therefore removes the dependence on the applied pressure, as this term cancels out[2].

For a typical power law *tail* PSD (Power Spectrum Density) of rough surface like $C(q) = Zq^{-2(1+H)}$ (we don't need to require the entire PSD to be a power law) the variance of slopes is $m_2 = \pi \int_{q_0}^{q_1} dqq^3 C(q) \simeq \frac{\pi Z}{2-2H} q_1^{2-2H}$, as a very good approximation unless the fractal dimension is strangely low. A typical fractal dimension nowadays considered realistic is $D = 2.2$ (most recent papers consider this, and Lorenz *et al* [6] give enough evidence).

Therefore, in this range we can approximate $\operatorname{erf}\left(\frac{1}{2\sqrt{G}}\right) \simeq \frac{1}{\sqrt{\pi G}}$ and (5) simplifies to

$$\mu \simeq \frac{\sqrt{8}}{4\sqrt{\pi}} \int_{q_0}^{q_1} \frac{dqq^3 C(q) \times \int_0^{2\pi} d\phi \cos(\phi) \operatorname{Im} \frac{E(qv\cos(\phi))}{(1-\nu^2)\sigma_0}}{\sqrt{\int_{q_0}^{q} dqq^3 C(q) \times \int_0^{2\pi} d\phi \left| \frac{E(qv\cos(\phi))}{(1-\nu^2)\sigma_0} \right|^2}} \tag{9}$$

where q_1 is the upper cutoff wavevector.

Within our approximation, for a given q we solve the integral under the

[2]Referring again to Grosch (1996) nevertheless, this is also only *approximately* true as a power law dependence with exponent -1/9 is often observed, and in some case even an increase of friction with load, which is not explained by any of the present theories, as far as I know, nor any theory has attempted to model the quite different dependence on normal load from wet and dry conditions.

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square root, and putting

$$R_1 = \frac{\int_0^{2\pi} d\phi \cos(\phi) \operatorname{Im} E(q_1 v \cos(\phi))}{\sqrt{\int_0^{2\pi} d\phi \left|E(q_1 v \cos(\phi))\right|^2}} \tag{10}$$

we obtain a further simplification to

$$\mu \simeq \frac{R_1}{\sqrt{\pi}} \sqrt{2m_2} = \frac{R_1}{\sqrt{\pi}} h'_{rms} \tag{11}$$

where we have written $\sqrt{2m_2} = h'_{rms}$ as it is correct for 2D surfaces where the gradient in orthogonal directions are uncorrelated. This equation looks already very similar to Persson [8] and Popov [13], respectively (2, 3), and we have reduced the original 4 integrals to just 2.

We will then show that a further very good approximation is to remove all integration processes and write

$$\frac{R_{1,appr}}{\sqrt{\pi}} \simeq \frac{\operatorname{Im} E(q_1 v)}{|E(q_1 v)|} \tag{12}$$

Indeed, we show this result is an extremely good approximation for realistic cases taken from Lorenz paper. We cannot provide a rigorous proof for this, but consider the simplest exact case, a Maxwell simple model of a spring of stiffness E connected in series with a damper of constant η, in the continuum version using the moduli rather than stiffness (see Popov [13], ch.15.7): the storage and loss modulus are

$$\operatorname{Re} E(\omega) = E \frac{(\omega\tau)^2}{1 + (\omega\tau)^2} \qquad \operatorname{Im} E(\omega) = E \frac{(\omega\tau)}{1 + (\omega\tau)^2} \tag{13}$$

where $\tau = \eta/E$ is a time constant. Computing R_1 and our approximate version

$$R_1 = \frac{\sqrt{2\pi}}{\omega\tau} \sqrt{1 - \frac{1}{\sqrt{(\omega\tau)^2 + 1}}} \simeq \sqrt{\pi} \left(1 - \left(\frac{3}{8}\omega\tau\right)^2 + ..\right) \tag{14}$$

$$R_{1appr} = \sqrt{\pi} \frac{1}{\sqrt{(\omega\tau)^2 + 1}} \simeq \sqrt{\pi} \left(1 - \frac{1}{2}(\omega\tau)^2 + ..\right) \tag{15}$$

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and notice that not only the first term is identical, but even quadratic and higher order terms have similar coefficients. For the Maxwell model, it could be shown that only at extremely high frequencies, those with no interest in practice for our application, the error could be at most 40%.

3. An engineering formula?

Cancelling the square root of π terms, our proposal is therefore to use an extremely simple approximation of Persson's theory

$$\mu \simeq h'_{rms} \frac{\operatorname{Im} E\left(q_1 v\right)}{\left|E\left(q_1 v\right)\right|} \tag{16}$$

This approximation seems coincident with what Popov gives in his book ([13], eqt.16.12), although Popov further suggests that in many cases max $\frac{\operatorname{Im} E(q_1 v)}{|E(q_1 v)|} \simeq 1$ in the intermediate frequency range. In our example cases below, it is R_1 that reaches a value just above 1, and not $\sqrt{\pi}$ because the loss modulus is never really greater than the storage modulus in this case. Moreover, one has to check if the velocity at which this maximum is reached, considering the truncating wavevector, is outside the "low velocity" assumption due to thermal restrictions.

Let us estimate this approximation with actual full results from Lorenz *et al* [6]. They consider 4 types of surfaces, having all Hurst exponent near 0.8 (hence, $D = 2.2$): three types of asphalt a, b, c and sandpaper. Their PSD looks in fact quite similar, except for a small shift in the multiplier Z so one wonders if really all this emphasis on the PSD determination is justified, at least considering that the theory we have at present essentially depend only on a quantity at very large wavevectors, which is arbitrarily fixed in the end. The obvious corollary of the postulate $h'_{rms} = 1.3$ independent on anything else (but let us assume for generality $h'_{rms} = c$ as this may well change in the future), together with out proposal (16), is that for the power law tracks tails $C(q) = Zq^{-2(1+H)}$, where we can easily show $Z = \frac{H}{\pi} h_{rms}^2 q_0^{2H}$ and hence

$$q_{1,cutoff} = \left[\frac{1-H}{H} \frac{c^2}{h_{rms}^2 q_0^{2H}}\right]^{1/(1-H)} \tag{17}$$

which seems a direct engineering formula requiring only the measurement of the slope of the PSD spectrum (at high wavevectors) Z, the Hurst exponent (which in practice most people assume 0.8) and the velocity, together with the viscoelastic loss and storage moduli as a function of frequency.

Journal of Tribology. Received February 01, 2017;
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We then make some actual estimate of our approximations, from Lorenz *at al* [6] paper. Consider the viscoelastic modulus of rubber compound C, which is detailed in their Fig.6, and scan carefully the data from the figure with a simple software (Engauge Digitizer). The storage and loss modulus is shown in Fig.2, and dashed lines are power laws at low frequencies, which are in fact those of interest, because in practice, as we assume low velocities $v < 1m/s$ (as otherwise the theory and experiments become too dependent on the heating process). However, we shall not need any power law approximation, and we see that the approximation is good in the entire frequency range.

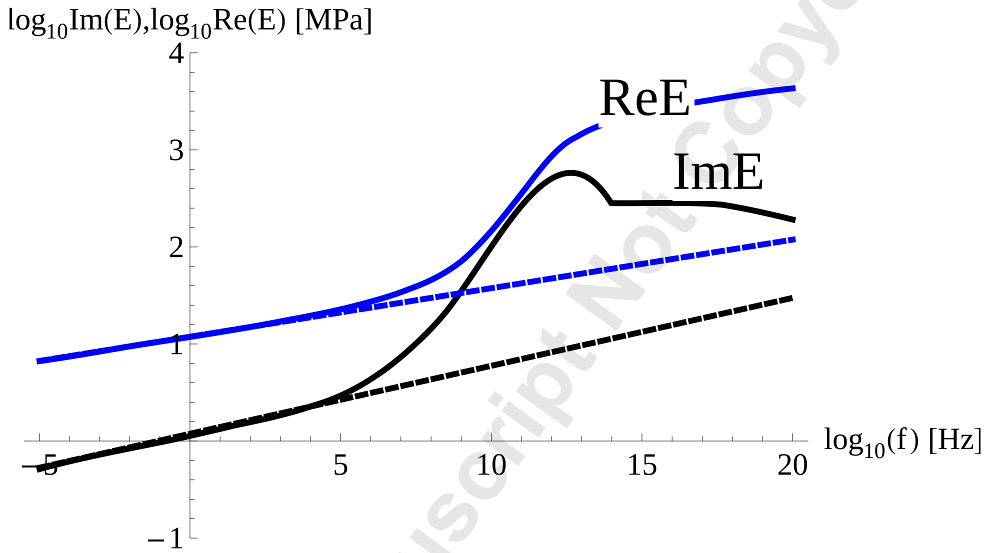

Fig.2 - Real (solid blue) and Imaginary (solid black) parts of the viscoelastic modulus in Lorenz *et al* [6] rubber compound C, together with power law approximations (dashed lines) at low frequencies. $\mathrm{Im}\, E = 10^{0.075} f^{0.07}$ and $\mathrm{Re}\, E = 10^{1.075} f^{0.05}$

Indeed, our approximation does not need the power law behaviour, as Fig.3 compares the (10), with our approximate solution (12) obtained from the full data. All the results are clearly extremely close, even above the maximum and this is irrespective on speed (results are given for $v = 0.01, 0.1, 1m/s$).

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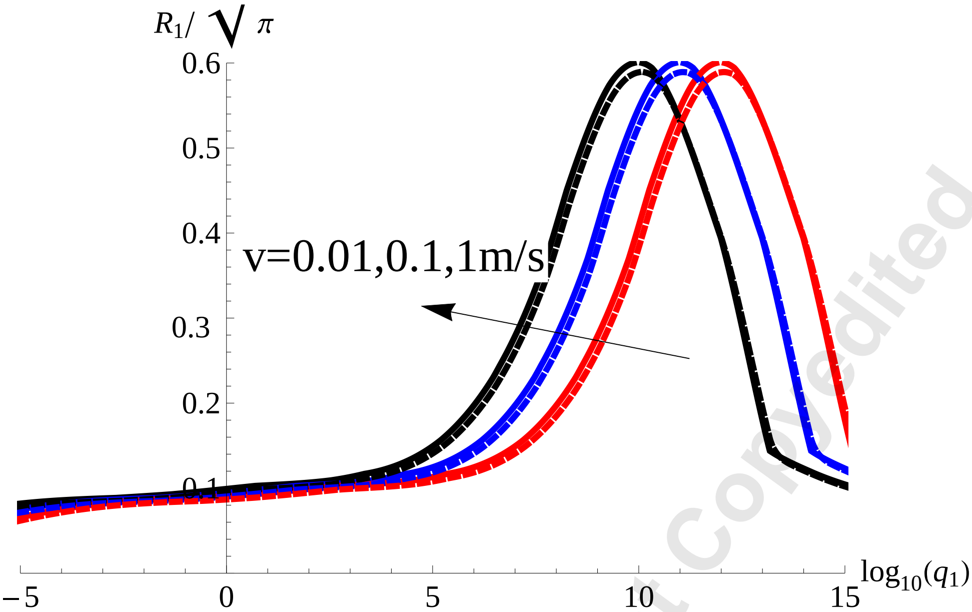

Fig.3 - A comparison of our approximate solution (12) $R_{1appr}/\sqrt{\pi} \simeq \frac{\mathrm{Im}\, E(q_1 v)}{|E(q_1 v)|}$ (solid lines) for the viscoelastic moduli ratio (10) $R_1/\sqrt{\pi}$, (dashed lines). The data are for decreasing velocities going from left to right (black, blue, red line), $v = 1, 0.1, 0.01 m/s$.

Next, to check our previous approximation in the integration, we extract from Lorenz paper in their Fig. 5a the rms slope for asphalt "a" as a function of truncating wavevector q_1 again with the same digital software.

The majority of Lorenz *et al* [6] friction data seem to imply a maximum coefficient of friction of the order 1, which is incompatible with the simple hysteresis loss model if we truncate the spectrum at wavevectors giving $h'_{rms} = 1.3$ as this corresponds to truncation in the power-law range of Fig.2 and therefore give a much lower friction coefficient.

But in our final proposal (16) we have made 2 approximations, one was to remove the full dependence on the PSD spectrum, and the other on the ratio of the moduli. Therefore, we need a comparison with a full calculation. This is possible by comparing for example our prediction with the results shown in Lorenz *et al* [6] Fig.17, again which we upload with the software, as a function of q_1, for $v = 1m/s$. The result is shown in Fig.4, showing an excellent agreement: the error is negligible, compared with so many other possible errors. Therefore, we conclude that the 2 successive approximations

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we have made are in fact quite justified. Therefore, the hysteresis loss can be obtained with a simple calculation, instead of the full calculation involving recursive integrations over many decades of spectrum of roughness. The implication is also obvious: is Persson's full theory really showing multiscale effects?

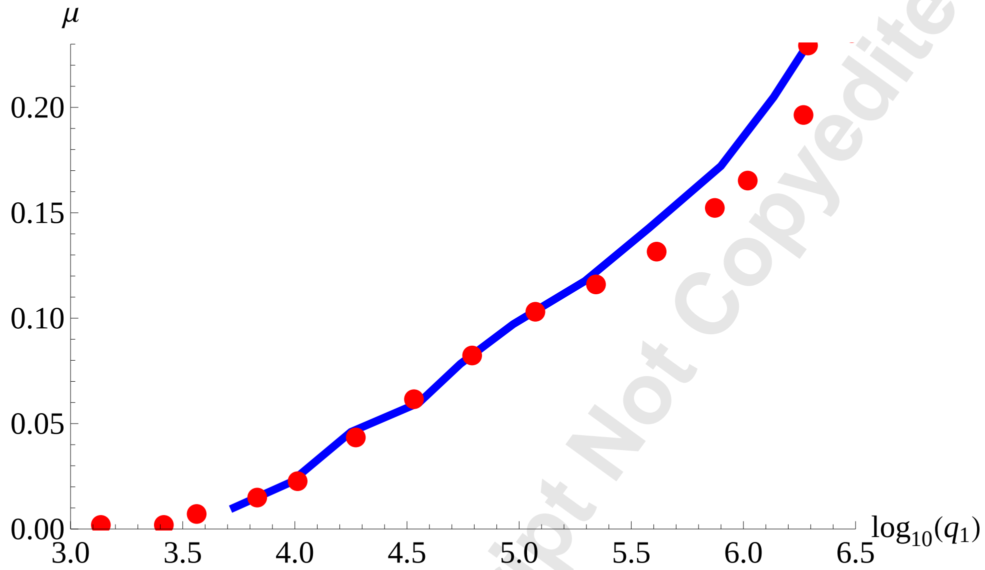

Fig.4 - Increase of friction coefficient with cutoff wavevector q_1 in Lorenz *et al* [6] (red dots), against our approximation (blue solid line) (16).

4. The two terms theory

We could further discuss the many additional contributions suggested by Lorenz *et al* [6] to fit the data with an "adhesion term", but we are not convinced much of the treatment because, according to Grosch, the two contributions (adhesion and hysteresis) should result in two maxima (see Fig.1), each related to its mechanism (molecular adhesion at much lower speeds, and hysteresis at much higher ones) whereas the experimental curves Lorenz *et al* produce remain always with a single maximum and therefore it is unclear if really two mechanisms are at play.

But most importantly, we disagree with the statement in the Discussion paragraph of Lorenz *et al* that "*there is noway to obtain the measured friction coefficient assuming only a viscoelastic contribution to the friction. That is,* *even if q_1 is chosen as large as physically possible, namely, of order 10^{10} m^{-1}, atomic length scale, it is not possible to obtain so high friction coefficient in the low velocity region ($v \simeq 10^{-3}$ m/s) as observed in the experiments"*. Instead, it is easy to show that, as $h'_{rms} \sim q_1^{1-H} = q_1^{0.2}$, if we let the rms slope increase for another 4 decades of spectrum to reach the "atomic scale" suggested by Lorenz *et al*, we could in principle have a rms slope higher by a factor $10000^{0.2} = 6.3$ whereas $\frac{\operatorname{Im} E(q_1 v)}{|E(q_1 v)|}$ would increase by a factor $0.6/0.18 = 3.33$ making a final friction coefficient of more than 3. This is represented in Fig.5 by the blue solid curve showing even too high friction coefficient, also at low speeds.

In fact, we find that a cutoff of $q_1 = 10^9$ m^{-1} which is represented by the solid black curve, fits the experimental data (red dots) quite well. This would suggest that the rms slope cutoff should be taken as $h'_{rms} \simeq 3.5$, but this is representative of the essentially fitting capabilities of these models. If one really had data with two maxima, and wanted to represent them with a single model, of course the problem would be more clearly defined. At present, this point seems therefore unresolved, as is the entire issue of the "appropriate cutoff".

Notice that we are responding to Lorenz et al's comment that it would not be possible to fit friction data with increasing the continuum theory down to atomic scales. We contradict this statement, but we are not suggesting that the theory should indeed be always truncated to atomic scale. As one reviewer pointed out, "rubber is a continuum only down to scale around 1 μm. At smaller scales rubber must be considered as a mixture of polymers and filler particles, and any theory operating at such small scales with the dynamic modulus of a rubber compound would be incorrect". However, Persson's theory suggested truncation is based on slopes and not on "scale", and hence all this requires further investigation.

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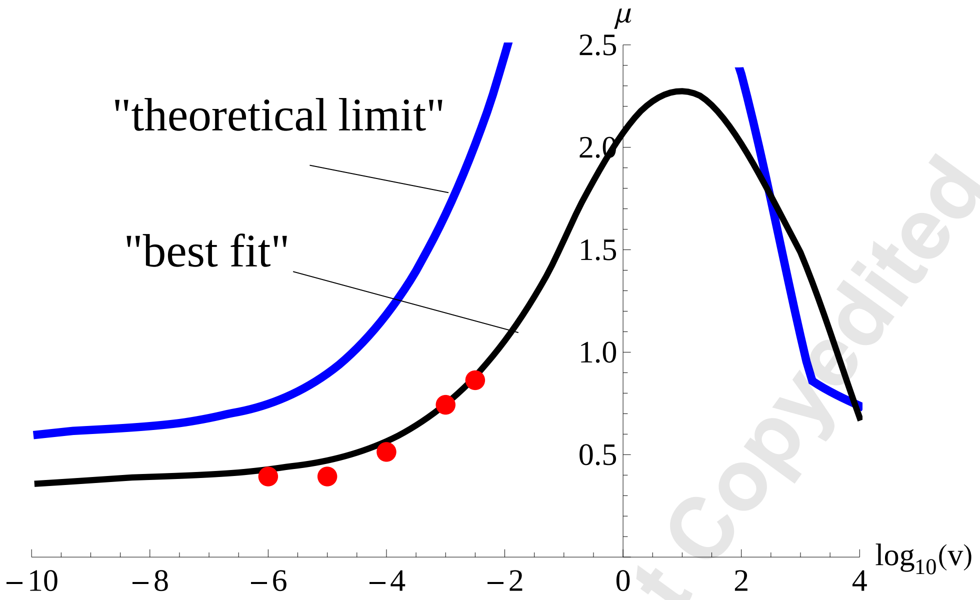

Fig.5 - Increase of friction coefficient with speed with different choices of wavevector q_1. Black line is the "best fit" with pure hysteresis loss contribution, which corresponds to a cutoff of $h'_{rms} \simeq 3.5$, whereas the solid blue line is the "theoretical limit" at atomic scale suggested by Lorenz *et al[6]*

5. Discussion: the challenges

In view of the present results, it is perhaps easier to judge in the future what Persson's theory (or at least, the pure hysteresis loss version) really shows in simple terms, since in fact it doesn't differ considerably from the simple Persson [8] single scale theory fitting model, or the Popov [13] simple derivation. Persson, in this 2001 theory [2], makes unclear arguments about the cutoff, at the same time saying that there should be no dependence on the cutoff, and also that there should be (see Appendix) – this is perhaps just a curiosity, but the problem has remained intact in the following 15 years. In Lorenz *et al* [6] we are told that the truncation cutoff q_1 should be chosen so that the rms slope becomes equal to 1.3: this is indeed the order of magnitude which was estimated in the single scale theory of Persson [8]. However, Lorenz *et al* do not agree that this cutoff should be put even farther, and seem to reach different conclusions than what we found with their own data: the fit could be made, provided we pushed the threshold to

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near the nanoscale: whether this makes any sense, or gives any real physical interpretation, is not clear at present, but certainly it makes a much simpler fitting equation. The "adhesion" contribution is probably at its early stages of development in Persson's theory, and it is very difficult to follow the various versions and arguments, especially as the key ingredients and proofs are then referred to in a reference (46), which then turns out to be "B. Lorenz and B. N. J. Persson (unpublished data)".

Anyway, the present note is really more on the simplification of Persson's theory, which in fact puts some arguments in favour of the conclusion of Popov's group (see Popov, *et al* [15]) that really only the very small scales count for hysteresis friction, but an important difference is that we reached this conclusion quite clearly from Persson's theory, and including also quantitative factors, and here we haven't made any use of the so-called "Method of Dimensionality Reduction" (MDR) [16]. Lyashenko *et al.* [17] have debated the conclusions of a previous paper by Popov's group, but the criticism is really more on the so called MDR method, which incorrectly computes the contact area for rough surfaces, than to the conclusion of the simple scaling of Popov [13] which in fact is similar to Persson [8] as we have here recalled.

Essentially, multiscale contact numerical findings (see Pastewka and Robbins [18]) suggest that the characteristic scale of asperity contacts depends only on geometry and is a very narrow distribution, close to constant. Hence, there is no reason to make a complicated multiscale model if the result is really a single scale asperity-scale.

There remains a real problem. Is it naif to even look for an "exact" theory of rubber friction on real surfaces like asphalt or concrete? How can we ever be able to considered all influencing factors on the friction coefficient, like surface porosity, polarity (expressed by free surface energies), brittleness, binder properties, lubrication, adhesion, dirty particles, viscoplastic, finite deformations, etc.? At the high slopes that all theories seem to postulate to explain the viscoelastic dissipation, how to include precise data about the materials properties (non-linear viscoelastic - even viscoplastic rubber behaviour)? If one insists too much on the multiscale aspect, one will not be able to include these factors. Hence, to spend resources on the purely academic question about multiscale aspect of the spectra, when we have then to impose arbitrary cut-off lengths in the spectra (and sharp one), is quite a debatable choice.

In general, Persson's theory is formulated for self-affine surfaces with Gaussian height distribution. Real surfaces are not necessarily self-affine

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with normally-distributed heights. Local slopes in contact points can be different from those calculated from PSD. II. Moreover, if the two terms theory is valid, then hysteresis friction theory by Persson is not relevant for low sliding velocities, where friction is dominated by adhesion: where is this further transition, in general? We are far from a "theory" of rubber friction, we simply have many fitting equation models.

5.1. Is there any meaning to a "sharp cutoff"?

In fact, one remark about the "sharpness" of the proposed cutoffs. While the multiscale theorists are all enthusiastic about the fact that "all scales below the cutoff" should be considered, they are all ready to consider than "no scale" beyond the cutoff should matter. This is a little curious. With a single scale model, one could attempt to condense the effects of "all scale below" and "all scale above" in some approximate sense. In the particular case of rubber friction, we think we have shown quite clearly that the multiscale effect is quite limited perhaps to a corrective factor to consider the actual contact area as a function of pressure – which gives indeed the correct pressure in the asperity scale. That asperities have almost the same size, it was already shown in the Greenwood-Williamson model. In these respect, it is remarkable to note that a theory which is considered equally competitive to Persson's theory (Kluppel & Heinrich [19], see also Heinrich & Klüppel [20]) is essentially based on a Greenwood-Williamson model, and yet it seems to work quite well.

We conclude that Persson's theory contribution is most important in the correct dependence of area with respect to load, which translates in the correct estimate of the pressure in the asperity, which is suspiciously high to apply any simple theory, so leaving all these refined models at most semi-quantitative.

At engineering scale, currently, a challenge is to implement a simple theory into a physically nearby "exact" theory in an suitable and effective FE-code, managing the rubber and road surface data identification, and then calculating with the FE-code the grip / skid characteristics of a complete tires (or other rubber parts). At present, many theories seem satisfactory in predicting the rolling resistance of a rolling tire, but not for the wet/dry skid behaviour (stopping distance) of a tire under real ABS-braking conditions. Perhaps we should therefore look first more into single scale models? The experiments of the so-called MDR theory in this respects may be at most

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suggestive, but attract the criticisms that the real contact area is incorrectly predicted.

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7. Conclusions

We have derived a simplified version of the multiscale Persson's theory, and we have proved that it leads to a result very much simpler than what the theory suggests, and a closed form engineering-level equation has been found, which suggests the crucial factor of just the rms slope of the spectrum, and the wavevector at which this is found. This is in line with previous theories from Persson [8] and from Popov [13]. We have confirmed that the approximation seems quite good with respect to full calculations, and provided simple explanation on why this is so, alternative from the qualitative derivations of from Persson [8] and from Popov [13]. We therefore suggested that the main progress of Persson's multiscale theory was the correct determination of the pressure at asperity scale (equivalently, of the correct coefficient of the area-load linear relationship), but that the quantitative aspects still are way beyond our understanding. In particular, the choice of the sharp cutoff remains arbitrary, and pushing it farther from what Lorenz *et al* [6] suggest, seems to be able to fit that data that otherwise Lorenz *et al* [6] suggest require a two-terms theory, involving many other assumptions and fitting parameters. In the present much simpler form, it may encourage further research into simple models, particularly on the sensitivity of the results to "scale-dependence", not in the sense of "sharp cutoffs" to be introduced in multiscale theories, but rather in corrective physics models in essentially single scale models which could correct for effects at scales both above and below the scale of choice.

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9. Appendix - the cutoff discussion in the original Persson (2001)

The problem of truncation cutoff was already considered in the famous paper where Persson (2001) introduced his contact mechanics theory. In that case, there is a confused argument for the function $P(q) = A(\zeta)/A_0$: first, it is recognized that for small pressures, the contact area goes to zero as $G >> 1$ and friction is independent on normal load. Persson argues that the contact area does not go really to zero as "For example, the shortest possible distances are of atomic length, ... or at a much larger length scale because of contamination particles, or trapped fluid or trapped pockets of compressed air, ... or a thin modified surface layer skin, or... the yield stress of the materials ... beyond that point the area of real contact stays constant".

However, even without an upper cutoff the friction coefficient given by Eq. 36 for a fixed sliding velocity v remain finite. This would not occur for full contact as

$$\mu \sim \int_{q_0}^{q_1} dq q^{1-2H} \sim q_1^{2-2H} \sim m_2 \tag{18}$$

since the integrand in Eq. 36 behaves as q^{-2H} (in fact it is rather q^{1-2H}) for large q, and the integral q^{-2H+1} (in fact it is rather q^{2-2H}) diverges if $H > 0.5$ (in fact, it diverges for all H!). However, when the correct asymptotic

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dependence $P \sim q^{-1+H}$ is taken into account the integral converges as q_1^{-H}: Persson clearly has forgotten a factor 1 as instead

$$\mu \sim \int_{q_0}^{q_1} dq q^{1-2H} q^{-1+H} = \int_{q_0}^{q_1} dq q^{-H} \sim q^{-H+1} \sim m_2^{1/2} \tag{19}$$

which does not converge and actually is exactly the inverse dependence of the contact area which he has just discussed not to converge. In fact, his calculations in Fig.11 and Fig.13 show a friction coefficient strongly dependent on the cutoff and even for realistically low fractal dimensions, are up to 5.