Discussion: "Extended Greenwood-Williamson Models for Rough Spheres" (Zhao, T., and Feng, Y. T., 2018, ASME J. Appl. Mech., 85(10), p. 101007)

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The authors have dealt with a problem that is quite common when nonconformal rough surfaces come into contact [1]. They state "To our best knowledge, however, no attempt has been reported to solve the contact problem with positive overlap between two rough particles." This discussion is aimed at highlighting that such work does exist and provide additional insight. Specifically, Ref. [2] deals with two contacting rough surfaces, one flat and the other hemispherical (similar to the case herein). But the work in Ref. [2] deals with *real* surfaces having the complication that both contain nonhomogeneous and anisotropic roughness properties. A procedure is given [2] where these properties had been reduced to parameters (spectral moments) that the GW model needs. It should be noted that what the authors call "GW, E-GW, and EP-GW" cases, are seamlessly handled in Ref. [2] without resorting to labeling, because the three regimes of elastic, elastic–plastic, and fully plastic are inherently imbedded in the JG model [32,43]. Because Ref. [2] uses the commonly known models of GW and JG, no new numerical procedures (such as "DEM") are needed. It just takes a thought process to fuse the said models for nonconformal rough surfaces, combining them in straightforward procedure outlined in Ref. [2]. Two cases are examined in Ref. [2]: a small load (0.1 N), and a high load (8 N) that puts the two cases in the elastic–plastic, and fully plastic regimes, respectively. The results are qualitatively similar to what appears herein but unfortunately, this paper does not provide sufficient information on the surface roughness properties (asperity density, radius of asperities, etc.) or spectral moments for quantitative comparisons to be made.

Seems that the authors are not aware that there is a closed-form solution to the GW model with a Gaussian distribution, which is given in Ref. [3]. That solution is mathematically exact, which eliminates the need for taking numerical integrations in the GW model, thus simplifying its use greatly.

There are a few other issues that the discusser differs with the authors. The authors refer to the GW "defects" which, perhaps, is an unseemly choice of a word. There are no physical flaws (or "defects") in the GW model per se. Under its assumptions, the GW model is sound. Unfortunately, the authors somehow do not see the *real* defects in the CEB [28] and ZMC [29] models that they use later. These models contain severe defects in the physics, of which there are quite a few. To the credit of the CEB model is that it was the first to offer an elastic–plastic model when no other model existed. Even this discusser had used the CEB model for the lack of a better one at the time, where in fact a closed-form solution had been derived for the CEB model as well [4]. One of the few main flaws of the CEB model is that it assumes a drastic and unrealistic jump going from elastic to fully plastic at the yielding onset of a single point. ZMC recognized that flaw, but presented an unsuccessful attempt to alleviate it by introducing a third-order polynomial template that has no physical justification. In fact, the ZMC model made things worse, because the transition from elastic to elastoplastic and fully plastic happens incorrectly as caused by that polynomial template (that is also not the only flaw in the ZMC model). It should be noted that even the CEB and ZMC models adopt the GW statistical approach by extending their results to the elastic–plastic regime—so if there had been a "defect" in the GW model, hereditarily it is transferred to all the models that adopted it. More details that discuss the CEB and ZMC models, along with their "defects" appear [5–7,32,43]. Superior and more recent elastic–plastic models have been developed. The CEB model provided great service at the time, and it has a historical value, but it needs to be retired, along with its offspring. The reader is referred to Ref. [21] that has a sound coverage and comparison of the more recent models.

Also, it is not clear what the MJG model is, as in Ref. [46] does not contain such a model. Assuming that the "MJG model" refers to the "updated JG model" in Ref. [46], it differs only marginally from the "JG model" (by about 3% at most). When statistics is involved (with or without averaging) hanging on to such negligible detail, does not really advance the state-of-the-art.

The purpose of the discussion is to make the reader aware of previous works and their virtues, favorable or not, at least from the discusser's point of view.

References

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