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Yvan Marthouret, Tony Zaouter, Florent Ledrappier, Guillaume Kermouche, Vladislav Yastrebov, et al.. Open Review of "Wedge indentation of elastoplastic solids-from single indentation to interaction between indenters". 2023. hal-04199877

**HAL Id: hal-04199877**

**<https://hal.science/hal-04199877>**

Submitted on 13 Jun 2024

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### Identifiers

Open Review

OAI hal-04199877

Reviewed Article

DOI 10.46298/jtcam.8945

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# Review of “Wedge indentation of elastoplastic solids – from single indentation to interaction between indenters”

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<sup>R</sup> Reviewer

<sup>E</sup> Editor

## Review of version 1

Permalink: hal-03517125v1

**Authors** We thank the reviewers for their careful reading and valuable comments on the manuscript. We have cautiously considered all of them and modified the new version of the manuscript accordingly as summarised in the actions taken below together with the following responses and the attached revised version.

### Reviewer 1 (Anonymous)

**Reviewer** Methods are appropriate and results appear to be interesting for the computational contact mechanics community.

**Authors** We agree with the reviewer regarding the availability of the data and scripts for a journal such as JTCAM. This should let the interested reader perform his own analysis of the data or create his own with the FEM scripts of the authors. This is crucial for reproducibility purposes. However, due to the size of the data generated by a FEM simulation, only the processed results of interest shall be provided (e.g., hardness). A FEM model could be generated by the scripts using any material and geometrical parameters, including those used by the authors, to produce a full data set on the FEM mesh if necessary. Action taken: Typical ABAQUS input files and associated scripts of FEM single and multiple indentions will be provided to an accessible repository. Simulation results will be stored in a HDF file format and take the shape of a Python nested dictionary. A “readme.txt” file will help the reader to understand how is the data sorted and can be processed and how to use the scripts.

### Reviewer 2 (Vladislav YASTREBOV)

**Reviewer** It is a very well written and well structured paper aiming at improving modeling of a single wedge and periodic-array of wedge indentations in plane strain formulation. The target applications are metallic sealing of turned surfaces and, potentially, cold metal rolling. First, the authors compare analytic models of material hardness for a single wedge-indenter with finite element simulations and demonstrate relative error maps by carrying out an intensive parametric study in terms of hardening exponent and initial yield stress. These maps put in evidence intervals of good

agreement between analytic and numerical results. However, the influence of the indenter’s angle, the only geometrical characteristics, was omitted in this study. Therefore, it remains unclear to which extent the obtained “agreement intervals” could be used for other angles of wedge indenters. My main criticism, however, concerns the numerical accuracy of the performed finite element simulations: my main concern is the mesh density which seems to me quite coarse. So I would invite the authors to justify their choice for the mesh density by weighty arguments, e.g. demonstrating mesh convergence (and probably also the convergence with respect to geometrical mesh characteristics). The second problem considered by the authors, represents a periodic array of similar wedge indenters with a distinct (from the single indenter) opening angle (this difference with the first part, in my opinion, does not allow the authors to carry out a more coherent normalization and comparison with the single-indenter study), where the authors simulate the nominal increase in measured hardness resulting from the interaction of initially separate indentation plastic zones. The results are consistent and quite insensitive to material and geometrical parameters; the latter conclusion could raise questions and, thus, requires some more elaborated justification. In overall, the paper is valuable for the community and contains interesting results which are presented in a novel manner; however, some discrepancies in the suggested hardness normalizations (see my detailed comments below) do not allow to judge on the overall consistence of the single- to multiple-wedge transition. In overall, I would invite the authors to justify their results in terms of mesh convergence and also in terms of a consistent transition between a single indenter and multiple indenters. It would help to construct an accurate and coherent framework which would lead to a better modeling of sealing materials. Additional detailed comments and relevant questions are listed below, most of them are intrinsically connected and most of them have roots in the lack of mesh convergence study. I invite the authors to address them too. Finally, since JTCAM promotes Open and Reproducible Science, I would invite the authors to share freely their data with the community.

**Reviewer** When the authors state “At this point the sealing is stopped but not ended”, it is not very clear what it means.

**Authors** In perfectly plastic theoretical framework (Hill [1950a], Salikhyanov [2019]), the sealing would stop at this point, except if the substrate is not semi-infinite. This means that a gap will remain between the two surfaces in contact, that cannot be further closed by increasing the vertical force. In the present article, the FEM simulations show a slow down of sealing (closing of the gap) but full closing of the groove eventually occurs. We agree that the sentence could be confusing. Action taken: In the revised manuscript, the sentence has been changed to “At this point the sealing is temporarily stopped and the gap is not entirely closed”.

**Reviewer** When describing their FE model, the authors write: “The mesh is especially refined near the contact zone, but is also sufficiently wide to approximate a semi-infinite solid.” I would invite the author to quantitatively justify this rather vague statement by concrete numbers and, probably, even a size-convergence results. Notably, for a particular (logarithmic) force-displacement relation in 2D problems, it would be interesting to specify what was the criterion used by the authors to demonstrate the convergence. Moreover, to justify accuracy of the obtained finite element results a mesh-convergence should be demonstrated. At some point (p.10) the authors admit “The finite element mesh might be too coarse to catch this [deviation of hardness for small indentation depths] regime properly as well” but they do not fix the problem. May be a non-homogeneous mesh could be used to obtain accurate results over the entire interval of considered loads.

**Authors** A mesh size convergence study was carried out for single and multiple indentation. The physical quantity considered for this investigation was the mean contact pressure (or hardness). It is computed over the entire simulation since the strain and strain field are supposed to be geometrically similar. For the sake of accuracy, we performed some single indentations with increasing and decreasing numbers of elements (i.e., mesh density under the contact). Only a weak mesh dependency on the hardness is observed, even for the coarsest mesh. The evolution of the calculated hardness as a function of the number of elements in the contact area is presented on Fig. 16 of the revised manuscript for single indentation for various materials. With a rather weak difference, especially when compared to possible errors coming from approximate models

of hardness, a comparatively coarse mesh was finally chosen for the study in order to minimize the computational time. Similar observations can be made in multiple indentation (Fig. 17 to 19 in the revised manuscript). Action taken: New paragraphs have been added in Sections 3.1 and 3.2 to discuss the effect of mesh convergence on the presented results. Appendixes A and B have been added as well, to present in more details the convergence studies performed for single and multiple wedges indentation.

**Reviewer** Having the same indentation angle  $\beta$  for the case of indentation with a single-wedge and a periodic array of wedges would allow the authors to compare the results before the plastic cores start to interact in the latter problem... and also to comment on the difference between isolated indenter and equally spaced indenters, and even to comment on the mesh convergence. However, the authors dealt with different indenter angles and different mesh densities which does not help in such an analysis. This choice was not justified, so I would invite the authors to comment on it in the paper.

**Authors** Most of the single indentation studies deal with a  $\beta = 20^\circ$  angle. It is the usual tip angle for indentation experiments since it is related to Berkovich and Vickers tips. Fig. 7 of the submitted paper extends the points made on  $20^\circ$  angles by comparing both models (REM and ECC) for  $5^\circ$ ,  $10^\circ$  and  $15^\circ$  angles. A good agreement between the 2 models and the simulations is found, indicating that the effect of the contact angle is well captured by the models. For double indentation (renamed multiple indentation), angles of  $5^\circ$  to  $20^\circ$  were studied but only  $5^\circ$  and  $10^\circ$  tip angles were presented in the paper. This choice was mainly motivated by the fact that in sealing applications, the angle of the grooves are expected to be relatively small. Action taken: Error maps for indentation angles  $5^\circ$ ,  $10^\circ$  and  $15^\circ$  were redrawn in the modified version of the paper. Some sentences have been rewritten or added in Sec. 4.1 for a better clarity.

**Reviewer** Comparing analytic models for material hardness with finite element simulations provide the reader with really valuable results. However, the lack of mesh convergence study does not allow us yet to use it as a reference result. Additionally, the grid discretization for the parametric study was not mentioned in the manuscript.

**Authors** A convergence study has been performed for both single and multiple indentation (see the comment 2 above). The sampling of the material parameters was not mentioned in the first version to make the paper easier to read. Though, we agree with reviewer that this lacks in the paper so it has been added to the revised version. A total of 450  $(n, \sigma_y)$  couples are studied, with a relatively fine regular sampling. Action taken: The grid discretization employed for the parametric study of single indentation has been explicitly specified in Sec. 3.1 of the revised manuscript.

**Reviewer** It would be reasonable to explain in detail how the authors compute the hardness ((i) either through computing the contact pressure or (ii) by dividing the reaction by the contact width). Because of a rather coarse discretization, errors could be considerable. Notably, it seems to me that sliding average filter used by the authors could be not very accurate. It will all depend on the way how the authors measure the contact width. I would invite the authors to comment on it in more details to make their results reproducible by the community.

**Authors** The reviewer is right. The way the contact pressure is computed is of primary importance in contact mechanics problems. However in the case of single sharp indentation, we can take advantage of the principle of geometric similarity that leads to a constant contact pressure during the whole indentation process. Its value is then computed by dividing the vertical force of the tip by the projected contact area, averaged on all simulation steps. This method should decrease the mesh dependency, hence permitting a relatively coarser mesh. Fig. 8 (blue curve) of the revised manuscript shows the evolution of the computed hardness during the penetration. A weak dependence is observed due to geometrical similarity, and the average value is kept. Sliding average filters are used only for multiple wedges indentation. In this case, geometric similarity does not hold anymore and the current (or apparent) hardness should be considered. We are thus bound to calculate an instantaneous hardness that becomes mesh-dependent. The coarsest acceptable mesh is used (to keep the computational time reasonable) so apparent hardness

obviously presents some minor artefacts due to mesh dependency (in the determination of the contact length for example). The sliding average filters are thus necessary to smooth the plots and focus on the general behaviour of multiple indentation. Nevertheless, the effect of this filter on the results is presented in the mesh convergence study. Action taken: Fig. 18 and 19 have been added in appendix B of the revised paper. To prevent all misunderstanding, some explanations were added for filters, mesh dependency and calculation method of single indentation hardness and multiple wedges indentation apparent hardness in the same appendix. A related paragraph in Sec. 3.2 has also been added.

**Reviewer** It is important for the reader’s understanding to show hardness results of FEA and analytical models (at least for one of them) whereas Fig. 5 and 6 show only the relative error. Notably, the reader cannot make his/her own judgment on the statement “Finally, its heterogeneity is a problem in itself while the relative error of the REM model can be compared to a constant offset.”

**Authors** We agree with the reviewer’s comment. Such data are lacking for the understanding of what have been done. An illustration of the performance of the two hardness models as a function of tip angle for typical material will be shown in the revised manuscript. The other data will be made available as supplementary materials. Action taken: In the revised manuscript, Fig. 5 has been integrated, and a related comment was written in Sec. 4.1. We also wish to notify that Fig. 7 was corrected, the contact angle used in the hardness formulas was incorrectly implemented. This is corrected in the revised manuscript and has no impact on the conclusions previously drawn.

**Reviewer** I wonder why in Fig. 10(a) and 11(a) the nominal hardness  $H^*$  does not saturate at 1 for  $L^* \rightarrow 0$ . Normally, the nominal hardness for a periodic indentation should tend, for small contact area, to the material hardness. The material hardness is well described by REM model as confirmed in Fig. 5 & 6. Therefore, it is surprising to see values of  $H^* < 1$ . When commenting the results, the authors remarked: “Let us note that  $H^*$  is not equal to 1 for  $n = 0.4$  at low contact ratio and the expected plateau-like evolution of single wedge indentation is not visible. (Fig. 12(a))”. However, the same statement holds valid for any other value of the hardening parameter  $n$ . I would invite the authors to justify this deviation. Here, it would be valuable to have a reference numerical simulation for a single indenter, in this case instead of using a model hardness, the authors could use the reference material hardness obtained by mesh-converged numerical simulations.

**Authors** The reviewer is right, as the dimensionless hardness should saturate at 1 for low contact length. The reason it occurs is a numerical issue related to the mesh-dependency of hardness at the beginning of the simulation. It is therefore more difficult to catch the expected plateau-like evolution of single indentation. Fig. 18 somehow highlights this mesh effect. The parametric study was based on a coarser mesh for saving computational time. The aforementioned sliding average filters were used to soften the mesh effect but it might make the results a bit misleading in this zone. We choose to present the results of the parametric studies and the coarse mesh with the understanding we have thanks to more refined simulation. Another reason of this observation is that high strain hardening coefficient make a quicker interaction response, decreasing the duration of the first stage (single indentation) and effectively restraining it to the least detailed part of the simulation. Finally, we are less interested in the beginning of sealing ( $L^* \leq 0.2$ ) so this inaccuracy is considered acceptable in the context of the present study, further considering that this regime possesses a well known solution from single indentation theory. The mesh convergence study discussed in a previous comment shows that this effect is attenuated as the mesh is refined, as expected. Action taken: Figs. 17 and 18 are added to appendix B, providing further information for the readers regarding the mesh dependence of our results. It is shown that the total error on the hardness–contact length curve due to the mesh size is less than 3% for the mesh used.

**Reviewer** The problem that I see in Fig. 10(a)-11(a), is that, apparently (not sufficient data is provided to let me judge), the indentation hardness of the simulation is sensitive to the penetration depth whereas the considered models for a single indentation do not have this dependence. Therefore, in my opinion, a detailed explanation of this dependence would be valuable, while, additionally accompanied with a mesh convergence study, which could be a source of this dependence. It



refers to my question #2.

- Authors** The principle of geometric similarity is well known in single indentation Hill [1950a]. As noted by Salikhyanov [2019], multiple wedges indentation triggers the interaction of slip lines fields of indenter tips. This interaction denied the necessary hypothesis of geometric similarity so that multiple indentation is effectively dependent of the penetration depth or, more accurately, dependent of any observable used to described the evolution of the indentation. As can be noticed in appendix B, the dependency is also observed for the most refined mesh that we consider converged, hence it is not a mesh effect. In this article we choose the contact ratio ( $L^*$ ) to follow the evolution of the indentation. Generally, the penetration depth that is mentioned by the reviewer is used instead (slip lines of Hill, Hertz contact...). In our case, the material is elasto-plastic with strain hardening so the studied indentation is accompanied by an elastic deformation of the bulk which is difficult to extract without arbitrary assumptions on this behaviour. The penetration depth (displacement of indenter relative to the undeformed non-contact surface) is hence badly defined. Indeed, the displacement of the indenter is polluted by the bulk elastic displacement and the non contact surface present some displacement due to both bulk elastic displacement and plastic flow between the tips. For these reasons we choose not to study the dependence in penetration depth but in contact ratio which is far more reliable in our case. Action taken: The principle of geometric similarity is described as well as its negation in multiple wedges indentation in Sec. 3.2 of the article. The appendix B of the revised manuscript confirms that the observed dependence does not come from mesh.
- Reviewer** In both models for a single wedge indentation, the main geometrical characteristics is the indenter’s angle  $\beta$ , its importance was also highlighted by the authors on page 9, Sec. 4.2.1. Nevertheless, the intensive parametric study carried out by the authors focused exclusively on material parameters  $n$  and  $\sigma_y$ . As demonstrated in periodic wedge indentation, the nominal hardness is very weakly dependent on the indenter’s angle. I wonder if the angle’s effect is present in a single indenter simulation and if yes, how can it be characterized.
- Authors** The indentation angle has indeed a great effect in single indentation. It is often highlighted in literature with hardness formula generally involving the indenter’s angle and some material mechanical properties Hill [1950a], Johnson [1987]. That is why we were somehow surprised by the weak dependence observed in our case of periodic indentation. We think that it can be partly explained by the fact that symmetric indentation is performed (angles are the same in both sides) and that the normalization by  $H_{mat}$  takes into account the majority of the effect of the contact angle on the response. Some further studies were made with asymmetric angles and it makes the angle one of the most influential parameter in the contact shape. Though, these results are not presented as they fall out of the scope of the present article. Action taken: Fig. 5 which was added, presents the dependency of the hardness for different angles.
- Reviewer** “Double wedge indentation” in reality is not a double-wedge but rather an infinite array of wedge indentations because of the symmetric (nominally periodic) boundary conditions. Therefore this term should be adjusted.
- Authors** It is totally true, and this double wedge represents the “unit cell”. This term is inherited from the first simulations where such conditions were not applied yet. In the literature, we found no occurrence of such experiments or problem with two or more indenters with the exception of Meguid et al. [1977] with two neighbouring flat punches. The authors proposed the term “co-indentation” that we personally find misleading. Action taken: The term was modified to “multiple indentation” or “multiple wedges indentation” in the revised version of the article.
- Reviewer** The authors write “Once a given contact ratio of  $L_* \approx 0.75$  is reached, the hardness starts increasing exponentially.” Within such a small interval it is impossible to distinguish between exponential or, for example, power-law increase. Therefore, I would recommend the authors to adjust this sentence or justify an exponential increase.
- Authors** We wrote “exponentially” with a weak common sense of “quickly increasing” but the confusion with a mathematical exponential increase is indeed possible and not justified. Action taken: The

term was changed with various words for “quick increase” without scientific connotation.

**Reviewer** The authors state: “The height of the bulk B has very little influence contrary to what was expected from the theoretical investigation of Hill [1950].” Normally, the slip line theory used by Rodney Hill is valid for rigid perfectly plastic materials only, so no hardening was taken into account. I would invite the authors to comment on it in a more elaborated way and notably to make a connection between a single indenter and Hill’s results in terms of substrate height  $B$ .

**Authors** Influence of bulk’s height is raised in perfect plasticity by Hill [1950b] and in perfect elasticity Johnson [1987], Sneddon [1995]. Hence, as we are using an elasto-plastic material, we assumed that this parameter should also affect the results. The hypothesis of semi-infinite (elastic) half-space is moreover often done in contact mechanics Greenwood and Williamson [1966], Johnson [1987], Salikhyanov [2019] letting the substrate’s height being of critical importance. With respect to these conclusions, we included this height in the parametric study but without specific conclusions on its influence. We should however qualify this lack of influence by the context of our study. The liner of industrial seals is generally less than 1 mm thick so the studied range of thickness is not very wide. Especially, contact length and bulk’s height are always comparable in our study so the answer of our contact problem does not depend on bulk height subject to such variations but a slight difference in behaviour may be observed compared to a taller bulk height (10 times more). Action taken: References to elastic theory with finite half-space were added. The parallel we made with perfectly elastic or plastic theories is further explained in Sec. 4.2.1 and the difference with our material elasto-plastic strain hardening material more clearly stated.

**Reviewer** To complement the discussion around Fig. 13 about pile-up and sink-in behavior with respect to the hardening exponent  $n$ , it would be interesting if the authors share their numerical results showing at least one of them. Otherwise, it is unclear if this well known behavior is recovered for a single indentation or not. The authors’ statement about higher loading: “Within this regime there is no more indentation pile-up and the materials flow up homogeneously to fill up the remaining free volume” makes me guess that the pile-up is formed in the initial stage, but it remains unclear whether sink-in is formed for strong hardening or not.

**Authors** For  $n = 0.3$  (consequentially  $n = 0.4$ ), a sink-in is observed in single indentation as shown in Fig. 14. Referring to multiple wedge indentation sink-in or pile-up is more difficult. Indeed, as explained in comment #8, the contact depth over penetration depth ratio  $h_c/h$  is badly defined for multi indentation. Moreover, some additional factors can be involved in this behaviour with for example the plastic flow of the material confined by the groove formed by two neighbouring indenters. It makes a comparison with classical comprehension of the pile-up/sink-in as presented in Bolshakov and Pharr [1998], Cheng and Cheng [2004] difficult as more parameters are at stake. For these reasons, we choose to not include the study of  $h_c/h$  ratio in the article and rather analysed the advance of the closing thanks to the contact area ratio  $L^* = L_c/W$ .

Action taken: The part about pile-up and sink-in was rewritten in Sec. 4.2.2., clearly assessing the observed results in single indentation and potential modifications by multiple indentations. Single and multiple indentations geometries are shown. It highlights that bearing contact model seems to be precise enough to describe the contact geometry. The link is also made with previous figures (10, 11, 12) showing that the bearing contact model is not so far from the finite element model in terms of global opened area.

**Reviewer** My final comment concerns the possible presence of a lubricant in the pocket made by neighbouring wedges. Entrapment of such a lubricant could strongly affect the apparent hardness and closing kinetics both in sealing and metal forming applications. It would be relevant, especially in view of sealing applications, to comment on this phenomenon, see [A,B,C]:

- A Azushima, A. and Kudo, H., 1995. Direct observation of contact behaviour to interpret the pressure dependence of the coefficient of friction in sheet metal forming. CIRP annals, 44(1), pp.209-212.
- B Bech, J., Bay, N. and Eriksen, M., 1999. Entrapment and escape of liquid lubricant in metal forming. Wear, 232(2), pp.134-139.

- C Shvarts, A.G. and Yastrebov, V.A., 2018. Trapped fluid in contact interface. *Journal of the Mechanics and Physics of Solids*, 119, pp.140-162.

**Authors** We thank the reviewer for this useful comment that opens new perspectives on our work. An incompressible entrapped liquid should totally modify the result with a faster passage to a global response of the bulk to an apparent flat punch stimulation and no further closing of the groove without a liquid interface squeezing out. The seals of interest in the present context are generally put up on flanges in dry conditions so that the presence of lubricant of any sort at the interface has to be prevented. This is the reason why we did not consider that this phenomenon deserved to be mentioned at first, though it is of practical importance in some applications. Obviously, this is more complicated to introduce in the numerical modelling.

Action taken: The following comment about the modification of our observed behaviour in case of coupling with a lubricant at the interface, referring to studies in this domain Azushima and Kudo [1995], Bech et al. [1999], Shvarts and Yastrebov [2018], was added at the end of Sec. 4.2.2 in the revised manuscript. “Our seals are put up on dry flanges and tighten immediately so the study holds for a dry medium. However the presence of lubricant is possible in similar applications. Submitted to pressure such fluid will at first partly flow out of the contact but could be entrapped in cavities according to surrounding surface deformation. In this case, which is not studied here, the plastic locking step is supposed to transform in a plastic-fluid locking step with the incompressible fluid preventing further deformation of the seal surface. As for the start of this locking, its study is not adapted to a 2D simulation because the start is triggered by the fluid entrapment. A 3D simulation of the contact would hence be necessary to describe such behaviour.”

**Reviewer** Minor comments

- Page 2: “The first stage is the interaction stage that corresponds to the interaction between the two neighbouring indentation strain fields.” → twice interaction, twice stage. Could be improved.
- Page 4: “Application of slip lines theory to interacting wedges were recently proposed by” → was
- Page 14: “Let us remember that the closing index is only related to the shape of the free surface for a given contact ratio.” → may be the verb “remind” would fit better here?

**Authors** Finally, the following references were added to the manuscript:

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- A. Azushima and H. Kudo. Direct observation of contact behaviour to interpret the pressure dependence of the coefficient of friction in sheet metal forming. *CIRP annals*, 44(1):209–212, 1995.
- J. Bech, N. Bay, and M. Eriksen. Entrapment and escape of liquid lubricant in metal forming. *Wear*, 232(2):134–139, 1999.
- A. Shvarts and V. Yastrebov. Trapped fluid in contact interface. *Journal of the Mechanics and Physics of Solids*, 119:140–162, 2018.



## Review of version 2

Permalink: hal-03517125v2

### Reviewer 1 (Anonymous)

**Reviewer** The authors have properly answered all the questions raised during the first review stage and the article is now acceptable for publication as is.

### Reviewer 2 (Vladislav YASTREBOV)

**Reviewer** The authors revisited and improved the manuscript by reformulating some text and by adding additional information and explications. However, I consider that some crucial information is still missing, notably (1) hardness maps, (2) a material-model formulation and (3) some explanations. Moreover, some statements could be further sharpen and some minor errors should be corrected. But the main problem with this contribution consists in inadequacy of the material model used for simulations. Such a strong criticism comes from my attempts to reproduce authors' simulations which highlighted some inherent problems of the model. The authors used a power-law hardening model, which implies an unbound increase of the yield limit (and thus of stress) with accumulated plastic deformation. This model could be valid for relatively small deformation interval but for the considered case, when total deformations could easily become very high, this model cannot even qualitatively represent the reality. As could be easily verified in the simulations presented by the authors, the accumulated plastic strains remain quite small (see Fig. 10 in the revised manuscript) and the volume changed drastically (we could easily deduce it from Fig. 14 in the revised manuscript, because the free border experiences no vertical displacement). Since a von Mises-type plasticity was used, this volumetric change is of elastic nature, which strongly contributes to the total reaction. If the authors show stress field, they could see that the von Mises stress could go up to 5% of the Young's modulus and overpass the initial yield stress by more than an order of magnitude! The absence of pile-up is thus dictated by the particular deformation regime not-preserving the volume. Finally, I could not fully reproduce authors simulations because the full description of the model is missing. The authors state that they use Hollomon's hardening model, which as far as I understand implies bulk modulus as a factor in the power-law increase of the yield stress. But it was not specified by the authors focusing exclusively on the parameters  $n$  and  $\sigma_Y$ . I used for my test simulation a value which is 10 times smaller than the bulk modulus... It is true that such a power-law model has been being widely used over many decades for “plastic-like” simulations, and the presented result, even though they seem to me rather hypothetical, as many others, have a right to be reported and published as a scientific contribution. However, in my opinion, the drawbacks of this power-law model and their discrepancies with the real behavior of materials and/or more realistic modeling should be highlighted by the authors to avoid misinterpretation of their results by the community. For example, the authors could analyze resulting stress field and comment on the adequacy of such results with respect to experimental data. Therefore, I would recommend to have another revision, which would make a clear statement in the field of multi-indentations. A detailed review with my comments and questions could be found below.

**Authors** We agree with the reviewer that the hydrostatic pressure induced by the elastic volumetric change is strongly involved in the increase of contact pressure. Therefore the bulk modulus can play a role when it comes to the full contact stage. This is a point that we did not raise in the first version of the paper but is now added in the new version. We thank a lot the reviewer for this useful comment that makes this paper stronger. However we did not see any issue in the von Mises stress calculation. As mentioned by the reviewer the Hollomon's law implementation is not detailed in the paper and some misunderstanding can therefore happen. It led likely the reviewer to implement the Hollomon's in a different manner, i.e. with a relation with the Bulk modulus. Hence we added a description of our implementation of Hollomon's law using the Abaqus Software. More specifically, it mostly consisted in tabulating the cumulated plastic strain as function of the yield stress within an isotropic strain hardening framework. Action taken: The implementation of Hollomon's law is described. A commentary about the volumetric change and the hydrostatic pressure is added.

- Reviewer** Previous comment: When the authors state “At this point the sealing is stopped but not ended”, it is not very clear what it means.
- Reviewer** Now, it seems to me that I spotted the origin of my misunderstanding, it is about the authors’ interpretation of the term “sealing”, still it is not clear. I see also some inconsistency in the text. The authors write: “The hardness (mean contact pressure) starts increasing up to the plastic locking process that happens when the two slip line fields fully recover. At this point the sealing is temporarily stopped and the gap is not entirely closed. The last stage of sealing can occur only if the resulting slip line field interacts with another one (through the presence of the substrate boundary or another asperity) that forces the materials to flow up again.” The first sentence says that slip-line fields interact (“fully recover”) which leads to hardness increase. At this point “sealing” (whatever it means) stops, it is still unclear. But according to the last sentence, the slip-line field needs to interact with a boundary or a slip-line field from another asperity... but it was already interacting, according to the first sentence, with neighbouring asperities’ field, doesn’t it? So, this paragraph remains unclear.
- Authors** It appears our previous changes was not enough. Reading again the description, it was indeed difficult to understand without illustration of Salikhyanov [2019] work. It would be difficult to add them to the present paper as they are already published in another journal. We thus provided a longer description of the evolution of slip-lines fields during the three steps. Action taken: In the second revision, a whole paragraph is dedicated to the description of the slip-lines field evolution.
- Reviewer** Previous comment: It would be reasonable to explain in detail how the authors compute the hardness ((i) either through computing the contact pressure or (ii) by dividing the reaction by the contact width). Because of a rather coarse discretization, errors could be considerable. Notably, it seems to me that sliding average filter used by the authors could be not very accurate. It will all depend on the way how the authors measure the contact width. I would invite the authors to comment on it in more details to make their results reproducible by the community.
- Reviewer** When the hardness is computed as a ratio of the reaction to the contact length  $H = F/a$  then, I agree with the authors, due to a discontinuous evolution of the contact length, which could be roughly represented by the number of identical segments (if we neglect in plane deformation)  $nh$  projected on the horizontal axis, when  $n$  is the number of elements in contact and  $h$  being their individual length. Then, at every new node coming in contact for infinitesimal increase in force from  $F^-$  to  $F^+$  such that  $F^+ - F^- \rightarrow 0^+$ , the hardness switches from  $H^- = F^-/a$  to  $H^+ = F^+/(a + h)$ . I consider that this second value is much more accurate than  $H^-$ . However, when the authors use a sliding averaging, they assume that the both values are equivalently correct. At the same time, since the authors do not search very accurate values of the hardness but rather try to demonstrate a three-fold increase in the hardness, probably, the sliding averaging could be used without harm.
- Authors** We agree with the reviewer comment. In this article, we focus on the three-fold increase in the hardness so that the small inaccuracy of averaging is not subject to consequences. Such method could be used to approximate the apparent hardness accurately but would need a finer mesh in this case. Action taken: The inaccuracy of the flat value of apparent hardness was recalled. The following comments have been made for the second review only.
- Reviewer** The authors state that they use Hollomon’s hardening model, which as far as I understand implies bulk modulus as a factor in the power law increase of the yield stress. But it was not specified by the authors focusing exclusively on the parameters  $n$  and  $\sigma_y$ . I insist on adding equations for the material model used by the authors. In the paper it is not clear how two parameters only  $\sigma_y$  and  $n$  can describe the hardening.
- Authors** It is true that Hollomon’s law does not describe perfectly the real behaviour of materials especially at high deformation. As the reviewer note, this law is often used for simulations despite its drawbacks. Indeed, such simplified law with only three material parameters ( $n$ ,  $\sigma_y$  and  $E$ ) enables parametric studies on a wide range of materials and is easily used for analytical models. There was a lack of introduction concerning the law and the chosen parameters for the study. Action taken: Eq. (3) introduce the law with a brief description. The choice of Hollomon’s law and its

studied parameters is commented p8. The use of the law and its discrepancy with real behaviour (high deformation) was recalled in the conclusion to avoid misinterpretation.

**Reviewer** “Contact pressure is taken at each node in contact...” normally, tractions/stresses are not nodal quantities except of the authors use the method of Lagrange multipliers and lower-order interpolation. Moreover, the contact pressure is not vertically oriented but normally to the contact surface, a projection on the vertical axis should be carried out.

**Authors** The reviewer is right and the apparent hardness can easily be obtained by  $F/L_c$ . In an asymmetrical case, it would be interesting to treat separately both side of the groove to normalize them by the adequate theoretical hardness thus resorting to a computation of hardness with contact pressure on each element. Action taken: The method of computation was corrected to  $\mathcal{H} = F/L_c$ .

**Reviewer** “The angle effect is shown on Fig. 5 for one material. The figure highlights that the two models have ranges of increased performance function of the contact angle.” What would it mean “increased performance function of the contact angle”?

**Authors** The sentence was not clear and has been changed. REM and ECC models depend on contact angle. Hence, there error too which is not observable on color error maps. So if REM model has generally less error, ECC can be more accurate for some contact angles. Action taken: The sentence was clarified.

**Reviewer** Page 11, Fig. 10: Step 1,2,3 are not commented. Maybe the corresponding points could be added in Fig. 9?

**Authors** It was clearly an oversight of our part! The purpose of Fig.11 is to make a parallel with the three stages of theoretical slip-lines and the three stages of simulations thanks to the area of plastic deformation. Action taken: The corresponding points were added to Fig.10. Fig.11 was described as well as the comparison and its obvious limitations.

**Reviewer** I do not agree with the following statement of the authors “when a fluid is present at the interface, the problem obviously becomes more complicated as three dimensional mechanical simulations accounting for fluid-solid interactions are required to describe such behaviour.” A 2D model could be successfully used within the set-up considered by the authors

**Authors** We probably overthought the presence of fluid at the interface thinking of a complete simulation to know if the fluid will stay entrapped during tightening or will just escape. In fact, it would be enough to reproduce the whole study with entrapped fluid to observe the modifications on our previous conclusions. We did not realised such study because our application focus on the fitting of the seal which is realised in dry conditions. Action taken: The paragraph about potential wet conditions was modified to highlight the potential differences with our conclusion about a dry case. References to 3D simulations were deleted. Previous papers about this problem were cited. Some spelling and typing mistakes were also corrected.

## Review of version 3

Permalink: hal-03517125v3

### Reviewer 2 (Vladislav YASTREBOV)

**Reviewer** The authors have satisfactorily addressed the majority of my comments, notably the addition of Fig. 5 in the revised version of the paper, which presents a helpful summary of hardness value maps for REM, ECC models, and FEM simulations. Therefore, I could recommend this paper for publication in JTCAM. However, before submitting the final version of the manuscript, I would like to suggest a few minor improvements that the authors may consider incorporating at their discretion:

1. The discussion around Eq. (3) (Hollomon’s power-law hardening) and later sections leave some ambiguity as to whether the deformation is elastic under yield stress  $\sigma_Y$ .
2. Around Eq. (8) the authors provide equations for a 1D rheology, but instead they could do so for

the general case of 3D stress and strain tensors.

3. In the discussion on page 14 about sink-in/pile-up, it might be worth mentioning the significant change in volume implied by the model used, which naturally influences the behavior of surface displacement between indenters.
4. The authors mentioned that they are ready to share Abaqus input files including meshes, however, in the present version of the manuscript no Supplementary Material was mentioned. Making available the datasets of material hardness could be also very helpful from the prospective of Open Science. All that could be fixed at the copy-editing stage when the paper is prepared to be ultimately published.

## Editor’s assessment (Alexander POPP)

The present contribution focuses on enhancing the modeling of both single wedge indentations and periodic arrays of wedge indentations. The proposed methods have the potential to find applications in tasks such as metallic sealing of face-turned surfaces and cold metal rolling. The initial manuscript underwent a rigorous review process, with two expert referees offering their assessments. The first referee provided an immediately positive review, primarily noting the absence of open data sets as an area for improvement.

However, the second referee, in the initial review, raised more substantial concerns and called for a major revision. These concerns included the omission of the indenter’s angle’s influence in the study, inadequate mesh density in the finite element simulations, and the need for a clearer transition between single indenter and multiple indenters.

In response, the authors conducted a first revision of the manuscript, successfully addressing the first referee’s concerns. However, the second referee’s critique of the material formulation, particularly the use of a power-law hardening model, remained a point of contention. Several minor comments were also addressed, but a major revision was still requested.

The second revision of the manuscript resolved the minor concerns raised by the second referee but did not adequately address the core criticism related to the material model. It was only in the third revision that the authors made significant improvements. This included the addition of further explanations to the manuscript, such as a summary of hardness value maps. These changes led to the second referee recommending publication.

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